SHL
WITHDRAWN

BIOGRAPHICAL MEMOIRS
OF FELLOWS
OF THE ROYAL SOCIETY

VOLUME 3

Digitized by the Internet Archive
in 2026

https://archive.org/details/biographical-memoirs-of-fellows-of-the-royal-society_1957_3

BIOGRAPHICAL MEMOIRS
OF FELLOWS
OF THE ROYAL SOCIETY

1957
VOLUME 3

A new series in continuation of
OBITUARY NOTICES OF FELLOWS OF THE ROYAL SOCIETY
Volumes 1-9 (1932-1954)

University College
Graham Library
Hospital Medical School

Published by The Royal Society
Burlington House
Piccadilly
London, W.1

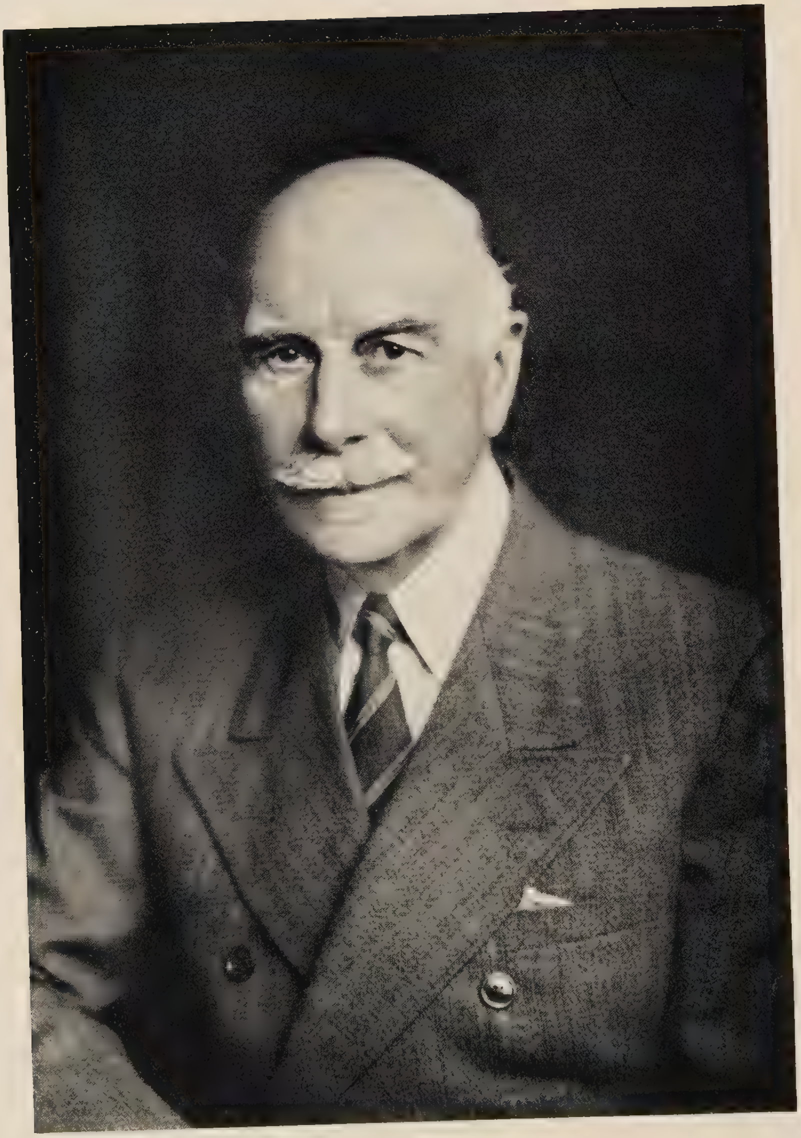
Published November 1957

PRINTED BY
HEADLEY BROTHERS LTD
LONDON, AND ASHFORD, KENT

CONTENTS

		PAGE
Alexander, Earl of Athlone	By Sir Charles Dodds ..	1
Norman Levi Bowen	C. E. Tilley	7
Frederick George Donnan	F. A. Freeth	23
John Evershed	F. J. M. Stratton	41
Arthur Felix	J. Craigie	53
Percival Hartley	Sir Henry Dale	81
Louis Vessot King	J. S. Foster	101
Albert Jan Kluyver	D. D. Woods	109
Gerald Ponsonby Lenox-Conyngham	Sir Edward Bullard ..	129
Henry Nicholas Ridley	Sir Edward Salisbury ..	141
Harold Roper Robinson	E. N. da C. Andrade ..	161
Henry Norris Russell	F. J. M. Stratton	173
William Wright Smith	J. M. Cowan	193
Frederick Soddy	Sir Alexander Fleck ..	203
Leonard Frank Spath	L. R. Cox	217
Arthur George Tansley	H. Godwin	227
Oscar Werner Tiegs	C. F. A. Pantin	247
John Sealey Edward Townsend	A. von Engel	257
John William Trevan	J. H. Gaddum	273
Hubert Maitland Turnbull	Sir Samuel Bedson ..	289
Hermann Weyl	M. H. A. Newman ..	305

A photograph of the subject is printed with each notice



Athlone.

ALEXANDER, EARL OF ATHLONE

1874-1957

It is unusual in Royal Society Biographical Memoirs to list the Public Appointments of Fellows like the Earl of Athlone, but it will not be possible for the reader to appreciate his very great versatility and industry unless the notice is prefaced by a certain amount of detail of his public life.

Born in 1874 he was the youngest son of the first Duke of Teck and Princess Mary and was thus a great-grandson of King George III. He was a brother to Queen Mary and, therefore, a great-uncle to the present Queen. Known up to 1917 as His Serene Highness Prince Alexander, he was educated at Eton, leaving there to enter Sandhurst and in 1894 was gazetted a 2nd Lieutenant of the 7th Hussars. He saw a considerable amount of active service and fought right through the South African war and for these services he was awarded the D.S.O. In 1904 he married Princess Alice, the only daughter of the Duke of Albany who it will be remembered was Queen Victoria's youngest son. From his marriage to the onset of the war in 1914, Prince Alexander was very active, and in view of the heavy commitments of the Prince of Wales (later Edward VIII) he and his wife found themselves very busily occupied on royal occasions. He found time during this period, however, to play a decisive part in the affairs of the Middlesex Hospital, but reference to this will be made in detail later.

His first major appointment was announced in the summer of 1914 when he was nominated to serve as Governor-General of Canada. Those who had the privilege of working with him immediately recognized this as a very excellent appointment, but in some sections of Canada the announcement was received with a certain amount of lukewarmness, as this was the first time that a member of the Royal Family had been appointed to the post. The war of 1914 called a halt to this appointment as Prince Alexander joined the 2nd Life Guards as a Major. He served with distinction throughout the war and was mentioned in dispatches twice. In 1917 he relinquished his title of Serene Highness and all the other styles of German nobility and was created Earl of Athlone and Viscount Trematon in the County of Cambridge. From the end of the war to 1923 followed an active period of service with various universities and charitable institutions and in this year he was appointed Governor-General of South Africa. At this period the affairs of the Union were extremely complicated and there is no doubt that the Earl of Athlone handled the situation with that delicacy and skill which his friends knew so well. So great was his success that at the termination of his spell of duty in 1927, General Hertzog requested on behalf of his Government that Lord Athlone should extend his Governor-Generalship for another period. In all he served eight years in this difficult position. He himself felt that his career in high Governmental posts had terminated and that he could devote the rest of his life to the pursuit of those medical and benevolent objects which were his real interests. It was not to be so, however, for in 1940 he was appointed Governor-General of Canada at the age of 68. He gave up his appointment in 1946 and again left behind him a reputation equal to that of his stay in South Africa. He made many visits to many parts of the Empire and entered into a period of activity that was maintained until shortly before his death.

This astonishing record of public service resulted in Lord Athlone being absent from the country for two long periods, the first from 1923-1931 and the second from 1940-1946, that is to say fourteen years in all, yet in the period up to 1923 and between 1931 and 1940 and after 1946 to his death he accomplished a number of undertakings in connexion with hospitals and universities which would have been a very creditable performance by itself apart from the public life which has been so sketchily outlined above.

The activities of Lord Athlone which would most interest Fellows of the Society can be classified under three main headings: Firstly his work for hospitals, secondly his work for postgraduate medical education and finally his work for the University of London. The first hospital institution for which he worked was the Middlesex Hospital. The Chairman was his brother Prince Francis of Teck, and when he died in 1910 Prince Alexander, as he then was, shouldered the whole of the responsibility of the hospital, assuming the Chair of the Board of Governors. He was responsible for a number of successful appeals and quickly established himself as a wise and beloved administrator in the hospital. He was quick to see that a profound change would have to come over the London teaching hospitals if they were to hold their own with institutions on the Continent, and particularly with those in the U.S.A. He recognized the importance of the teaching of science in the London Medical Schools and also he realized that any school that would claim to have a future must contribute to the general knowledge in the form of medical research. He played a vital part in raising funds for the cancer research laboratories of the Middlesex Hospital and also by his contacts was able to help with the provision of funds for the Medical School in general. On his return from the 1914-1918 war he again resumed the Chairmanship of the Middlesex Hospital and held this until his departure as Governor-General of South Africa. At this time the Middlesex Hospital Medical School was undergoing a very rapid change from the modestly equipped pre-1914 Medical School to the modern, largely rebuilt and totally re-equipped Medical School, that we know today. The Dean of the Medical School at this period, Mr Alfred Webb-Johnson (now Lord Webb-Johnson) was perhaps one of the shrewdest and most far-sighted Deans of his time and he saw the necessity of developing close relationship with the University of

London if the Medical School were to survive in the future. Accordingly, a number of scientific academic departments were created in the School with endowed Professorships at their head. Lord Athlone, together with Webb-Johnson, played a decisive part in interesting the numerous benefactors who were responsible for providing the funds for these developments. He left the Middlesex permanently in his debt and his memory is still fresh in the Hospital and Medical School.

One of Lord Athlone's first undertakings on behalf of medical education from the official point of view was his acceptance of the Chairmanship from the then Minister of Health Dr Addison (later Lord Addison) of a Committee to consider postgraduate medical education. This Committee was known as the Postgraduate Medical Committee and was appointed on 26 January 1921. The date of the report is May 1921, and a study of it will show that the Committee held not less than 26 meetings. Those who have had experience of committees of this degree of seniority will recognize in this speed an almost unheard of record. The members of the Committee themselves paid public tribute to the debt they owed to their Chairman and it is generally accepted that Lord Athlone was very largely personally responsible for the businesslike way in which the project was dealt with. In University medical circles this report is regarded as being one of paramount importance in the development of the University. It is extensively referred to in the Goodenough Report and there is no doubt that of the many achievements to arise out of this Report is the London School of Hygiene and Tropical Medicine. The British Postgraduate Medical School at Hammersmith can also be regarded as having its foundations in these deliberations. Lord Athlone also extended his interest from medical education to nursing and he played a considerable part in the setting up of the official governing machinery of the nursing world as we know it today.

Apart from being concerned with postgraduate medical education, Lord Athlone served directly the cause of the University of London by acting as its Chancellor from 1932 onwards. He came at a critical time in the history of the University when internal politics had caused great strife in this complex organization. Space does not permit a full review of the position of the University prior to 1931, but perhaps the following brief notes will help the reader to picture the difficult problem facing Lord Athlone when he took this onerous task. The sixth Chancellor, Lord Roseberry, died in 1929 just as the University of London Act of 1926 was being implemented. This Act postulated a series of new statutes which were designed primarily to centralize the University of London and give it much greater influence over the conduct of the various schools of the university. These previously had enjoyed very considerable autonomy and very naturally regarded the new statutes with grave suspicion fearing that they would become subjugated to a rule from a central authority. The far-seeing principle behind the Act of 1926 was to constitute a central authority that could deal with bodies such as the Government when negotiating for financial support. As the result of these statutes a court was created and this represented the University when dealing with bodies such as the Government Grants Committee, local authorities and so forth. Many of the independent schools felt that their freedom was being menaced and the most vociferous amongst them were the medical schools. It must be remembered that these were amongst the oldest constituents of the University of London, having been in existence from, in some cases, the middle of the eighteenth century. The problems facing a new Chancellor were, therefore, of the most difficult and complicated character. A new appointment was made in the person of Earl Beauchamp, but after encountering very great difficulties he resigned office in 1931. Those responsible for a new appointment passed through a very anxious period, particularly in view of the proposed move to Bloomsbury and the celebrations of the centenary of the foundation of the University in 1936. After a very careful review of names Lord Athlone's was put forward as the only person who it was felt could tackle this vast and difficult problem. Under his Chancellorship the University moved without hesitation forward to the great position that it holds today. The feeling of irritation in the independent schools such as the medical schools very quickly disappeared under his personal charm and fairness.

Being resident in or near London he spent a great deal of time at the University and was present at practically all its official and social functions. He was accompanied always by his wife, Princess Alice, and their gracious presence at University functions will be remembered by all who were fortunate enough to experience it. Many Chancellors only appear on graduation day, but Lord Athlone showed a day to day interest in the University very much to its advantage. No greater tribute to Lord Athlone as Chancellor of the University can be found than in the Report of the Principal for the year 1954-1955:

'The duties of a Chancellor vary greatly from University to University. In some cases they seem to be both formal and limited, perhaps even confined to presiding over the annual graduation ceremony. In a federal University, however, the Chancellor is inevitably cast for a role of far greater significance because he symbolizes in his own person the essential unity of the University amidst obvious diversity. This is above all true of the University of London where the federated institutions are geographically so scattered and often so specialized in their academic pursuits. During the past twenty-three years it has been our great privilege to have as our Chancellor the Earl of Athlone and his resignation last January came as a great blow to us all. Our deep sense of loss was expressed in the following message which was sent to him jointly on behalf of the Court, the Senate and Convocation:

"The University has learned with profound regret of your decision to resign from the office of Chancellor. Throughout the long period since your appointment in 1932, you have presided over the affairs of the University with a dignity which can rarely have been equalled. We recall with deep gratitude the continuing interest which you have displayed in all aspects of the work of the University and we wish to place on record our vivid appreciation of the many services which you have rendered to it. Above all, we would thank you for the stimulus which you have given to our students by your presence at their different functions and for the unfailing friendliness and courtesy which you have shown in your relationships with all members of the University.

It has been our great fortune that, in the execution of your official duties, Her Royal Highness Princess Alice has constantly been at your side and we would wish to take this opportunity of paying our tribute to the grace and the distinction which she has added to the ceremonies which are a visible expression of the corporate life of the University.

Perhaps we may be permitted to say that you and Her Royal Highness have won a unique place in our hearts and it is a matter of great sadness to us that your official connexion with the University has now come to an end. May we assure you that you will both long be remembered by all members of the University with deep affection as well as with profound admiration."

'Well might we have said:

"Here was a Caesar! when comes such another?" '

The University benefited very greatly by this association with the Royal Family and there is no doubt that it owes a very great debt to Lord Athlone for persuading the Queen Mother to take the Chancellorship on his retirement.

Princess Alice became Chancellor of the University College of the West Indies and this again brought another link between that Institution and the University of London which was the parent body.

We can see, therefore, that the Earl of Athlone devoted his whole life to the service of mankind and that despite his heavy commitment of official duties he was able to make very singular contributions to medical education, University education in general and science and research in particular. As a personality he was most attractive. Tall and handsome in his youth, extremely dignified and impressive in his middle and old age, and one of the easiest of men with whom all and sundry could enjoy association. He was endowed with that royal gift of an excellent memory for names, faces and occasions and on re-visiting some of the old places in which he had held high office he used to astonish everyone by his unfailing recognition of people whom he had not seen possibly for 20 years. He inspired the deepest loyalty in all who came in contact with him and it was probably because of this that he was so successful. He never spared himself at all and the writer has a very vivid remembrance of him sitting in the Secretary-Superintendent's office of the Middlesex Hospital, signing with his own signature several thousand letters of an appeal for funds for the hospital.

It is a pleasure to acknowledge the help of Lord Webb-Johnson, and I am particularly indebted to the Principal of the University of London, Dr D. W. Logan, for his guidance concerning the University of London.

E. C. Dodds

Norman L Bowen

NORMAN LEVI BOWEN

1887-1956

Norman Levi Bowen, Research Associate of the Geophysical Laboratory of the Carnegie Institution of Washington, and one of the great pioneers in experimental petrology, died in Washington D.C. on 11 September 1956 in his seventieth year. He was born at Kingston, Ontario, on 21 June 1887, the younger son of William Alfred Bowen, a Londoner by birth, who had come to settle in Canada. Bowen attended school in Kingston and in the autumn of 1903 entered Queen's University, registering in the Faculty of Arts. There he took an honours course in chemistry and mineralogy, graduating with the degree of M.A. in 1907 with the University medals in both subjects. In the same year he entered the School of Mining completing the B.Sc. degree course in 1909 in mineralogy and geology. Among his professors at Queen's were R. W. Brock, subsequently Director of the Geological Survey of Canada, M. B. Baker and W. Nicol. E. L. Bruce and W. L. Uglow who later achieved distinction in geology were among his contemporaries. During his University career at Kingston he became engaged in field work for the Ontario Bureau of Mines, first in the summer of 1907 with Brock on a geological survey at Larder Lake, and in succeeding field seasons with M. B. Baker at Lake Abitibi (1908) and in the Gowganda Lake area in 1909, working there under the general supervision of A. G. Burrows.

The results of his studies in these years appeared in two contributions, the first as a student's paper in 1909 on diabase and aplite of the cobalt-silver area —which was awarded first prize by the Canadian Mining Institute and the President's gold medal; the second appeared in 1910 in the *Journal of Geology*.

In the course of his field study he had been particularly struck by the occurrence in the black diabase sills of the area of pale pink aplite dykes and granophyre patches. The first paper on these was largely chemical in its approach, but the second surveyed the problem both from the field and laboratory standpoint, and he was then led to the conclusion that these acid veins and patches were formed by hydrothermal action at the contacts of the diabases with slates. This conclusion is of special interest in view of Bowen's later contributions to the problem of magmatic differentiation based on his experimental phase equilibrium studies on silicate melts.

Five years later he was prompted to refer to this early work of his student days in the following terms:

'In the original paper [1910] it was considered that the surrounding sediments played an important part in the formation of the granophyric bodies at the upper surface of the sills. This opinion was arrived at principally because of the difficulties of picturing any process of pure differentiation whereby a quartzose rock could be formed from basaltic magma. With this difficulty removed the writer has no hesitation in concluding that the granophyre and the micropegmatite interstices of the diabase were formed after the manner detailed in the present paper [1915], and that interchange of material between the granophyre and adinolized sediments was a subsidiary process contributing to the soda-rich nature of the border phases.'

Though he was to spend three more summers in the field, the season of 1909 was the last that Bowen spent in north-eastern Ontario. He had now spent three summers in the bush, for the most part working independently, in country where in the summer the canoe was the preferred means of transport, bringing in his supplies and moving his camp as the work required, and by this time he must have become quite expert with a canoe and quite accustomed to packing.

In the autumn of 1909 he proceeded to the Massachusetts Institute of Technology with an 1851 Exhibition Scholarship. Bowen's decision to proceed to M.I.T. was not taken without much reflection. In a short autobiographical sketch he reveals that it was his hope to take his graduate studies in Norway under Professor J. H. L. Vogt. To the young geologist keen on a physico-chemical approach to his problems the name of Vogt made a great appeal, but his contacts with that savant were not encouraging and 'with the fading of the Norwegian vision' his eyes turned southward to M.I.T. where he was enrolled under Daly, Jaggar, Warren and Lindgren in geology and Noyes, Lewis and Bray in chemistry.

The summer field seasons of 1910, 1911 and 1912 found him again in Canada; in 1910 for part of this period along the north shore of Lake Superior and later in a salt producing area of the south-western peninsula of Ontario. In the Thunder Bay area (Lake Superior) he studied with his own party the silver-bearing veins associated with diabase sills, and for this assignment his experience in the Gowganda district must have fitted him well. In his report on the area he observed the presence in the diabase at places of collections of feldspar crystals resulting in 'bubble-like masses', two feet to twelve feet in diameter, of rock that is anorthosite in composition and suggested that an extension of the process might result in the production of bodies of anorthosite.

This suggestion was to be greatly developed and elaborated in later years in his discussions on the genesis of monomineralic rocks and on the problem of anorthosites in particular.

The field seasons of 1911 and 1912 were spent in British Columbia surveying along the main line of the Canadian Pacific Railway for the Geological Survey of Canada; the first as an assistant to R. A. Daly working in the

Shuswap sheet, and in 1912 with his own party under the general supervision of C. Camsell in the Frazer River Valley from Lytton to Vancouver.

Some of the work of these surveys was incorporated in the guide book to this part of the Canadian Pacific that was issued for the 1913 session of the International Geological Congress in Canada.

In his second year at M.I.T. (1910) Bowen proceeded to the Geophysical Laboratory of the Carnegie Institution of Washington to use its facilities for a phase equilibrium study of a silicate system.

Here he worked out the liquidus-solidus relations and solid solution phenomena at high temperatures in the binary system $NaAlSiO_4$-$CaAl_2Si_2O_8$, and this research he was able to use towards his Ph.D. degree at M.I.T. to which he returned in 1911, gaining his degree in 1912. The time for decision on his future career had now arrived. Opportunities for employment in various geological fields were not lacking, but the opening provided by the Geophysical Laboratory at Washington where his ability and resource were at this time well appreciated, finally attracted him. It represented for him a complete fulfilment of the wish to avoid a forthright choice between geology and chemistry. So he became a regular member of the staff of the Washington Laboratory and there in pleasant and stimulating surroundings he was to spend his life, except for one brief, and another somewhat extended excursion into University work.

The three years succeeding his move to Washington were to see the execution of a group of experimental studies which revealed his exceptional skill as an experimenter and as an exponent of phase equilibrium theory. They were in his hands to prove of critical importance to petrology.

Day and Allen in 1905 in the first publication from the Laboratory had broken new ground in their pioneer study of the isomorphism and thermal properties of the plagioclase feldspars. Since the completion of those studies the facilities for silicate investigation at high temperatures had greatly increased and among the techniques developed had been that of the quenching method (Shepherd & Rankin 1909) for the study of melting phenomena in substances forming viscous liquids on melting.

A detailed investigation of the system albite-anorthite with these facilities now available to him was undertaken by Bowen in 1913 and has become a classic study. He was able to demonstrate an unbroken series of solid solutions in the plagioclase series, establish the positions of the liquidus and solidus and calculate the latent heats of melting of anorthite and albite and from the experimental data discuss the importance of zoning of crystals in the crystallization of plagioclase in magmatic melts.

There followed the study of the system MgO-SiO_2 (1914) (with Olaf Andersen), the systems diopside-forsterite-silica (1914), and diopside-albite-anorthite (1915). These systems for study had been shrewdly chosen, for they embraced not only the most important solid solution series (the plagioclases) among rock-forming minerals, but included end member representatives of a group of the chief minerals building basic igneous rocks.

It was these studies which provided the basis for the memoir on 'The later stages of the evolution of the igneous rocks' which appeared under Bowen's name at the close of 1915, for the experimental findings of incongruent melting phenomena, solid solution zoning and gravitational movement of crystals in silicate melts were brilliantly applied to the elucidation of the problem of igneous rock variation.

The 1915 memoir was of outstanding merit and established Bowen's position at the early age of 28, as an international figure in the field of petrology. His interest in the special character of monomineralic igneous rocks led him at this time to a study of the problem of the anorthosites (1917) interpreted by him as the most important representatives of this class generated by the process of collection of crystals under the influence of gravity with examples drawn from the Adirondacks and Quebec.

For the Adirondack anorthosites this was a solution which was a radical departure from the concepts prevailing at the time; it was discussed from the standpoint of the field geologist by Cushing and Miller and at a much later date after detailed survey by Buddington (1936) who drew a distinction between anorthosites of stratiform sheets and *massif* anorthosites of the Adirondack type, the former conforming more strictly with the segregation processes envisaged by Bowen.

The entry of the United States into the first World War in 1917 brought about a complete re-orientation of the programme of research by the staff of the Geophysical Laboratory.

The resources of the institute were deflected in large degree to the vital problem of producing within the United States from American materials both a quality and a quantity of optical glass adequate for national needs. Bowen co-operated in this effort and moved for a time to the glass plant of the Bausch and Lomb Optical Company.

A by-product of these wartime studies appeared in several papers by him which directed attention to the significance of glass-making processes to the petrologist.

When the War came to an end he resigned from the staff of the Laboratory, returning to his Alma Mater, Queen's at Kingston as Professor of Mineralogy (1919).

Among his pupils at this time were J. W. Greig, later to be attracted to Washington and to be associated with Bowen in silicate research, and J. E. Hawley now Research Professor of Geology at Queen's.

But his sojourn as university professor was brief. The department at Queen's then possessed one microscope and negligible facilities for research of the type he had been accustomed to, and before long the persuasive approaches of Dr Day, Director of the Geophysical Laboratory, achieved his return to Washington.

In the following year (1921) he was to provide an experimental approach to the problem of diffusion in silicate melts. In these experiments he studied the diffusion against gravity of a heavy liquid into a lighter liquid using diopside and plagioclase melts and studying the extent of diffusion by measuring the refractive index of the glass across the contact of the solidified melts.

He was led to conclude that the diffusivity was such that it could not be considered as playing a significant role in the formation of border phases about large bodies of igneous rock, a role that had been assigned to it in some earlier speculations. In the time available in a cooling magma such action could not be considered possible, though diffusion was competent to explain the formation of reaction rims of moderate thickness about inclusions in magma.

About this time, too, he presented his conceptions on reaction processes in magmas during the process of cooling, led to this by his detailed studies of phase equilibria among silicates.

Thus in a paper of outstanding significance 'The reaction principle in petrogenesis' (1922) he demonstrated the importance of the reaction relation between liquid and crystal phases and in contrast the relative unimportance of the eutectic relation. He defined the continuous and discontinuous reaction series, the existence of which controlled the crystallization and differentiation of the rocks of an igneous sequence and interpreted on this basis the real significance of Rosenbusch's normal order of crystallization, an empirical rule which hitherto had defied theoretical interpretation.

Petrogenic theory had now passed beyond the stage where the conception of eutectics could be regarded as of any considerable service, for in contrast to the flexibility introduced by the reaction mechanism a eutectic system was rigid 'for whatever the conditions the eutectic is the goal of all liquids; all attain it; none pass it; it is a fen of stagnant waters'.

These studies were applied in the same year (1922) to the interpretation of the phenomena of assimilation in igneous magma. He provided the first physico-chemical treatment of the behaviour of inclusions in magma—that magmas may incorporate considerable quantities of foreign inclusions both by the method of reactive solution and by reactive precipitation.

These actions were recognized as an emphasizing of normal processes possible in the absence of foreign matter, and he was led to the view that it was doubtful whether the presence of foreign matter was essential to the production of any particular type of differentiate.

In collaboration with Greig a re-investigation of the system Al_2O_3-SiO_2 led to the discovery of a new binary compound $3Al_2O_3.2SiO_2$, subsequently named mullite from its occurrence in xenolithic assemblages in the tholeiitic pitchstone sheets of Mull.

They had been led to this study from Bowen's observations of the setting free of corundum as a reaction phase in aluminous sediments brought into contact with magma under conditions where there was ample silica to form sillimanite.

There was implication that the reported congruent melting of sillimanite might be in error.

The experiments went much further; they established that the 'sillimanite' of the melts was in fact the new compound and that the sillimanite composition, far from constituting a maximum on the curve of melting temperatures in the system, begins to melt at 1545°C. Mullite on the other hand melts only when a temperature of 1810°C is reached.

The inference was clear that refractory 'sillimanite ware' should be adjusted in composition to the much more refractory composition of the 3 : 2 oxide ratio for mullite.

A signal advance in refractory technology had been recorded. That mullite itself melted with dissociation releasing corundum gave the clue not only to the release of this mineral at magma contacts already noted, but also to its appearance in glass manufacture at the contact of molten glass with its clay container.

In co-operation with Morey a study was made of the equilibrium relations in the oxide system of the soda-lime glasses, particularly the field within the system Na_2SiO_3-$CaSiO_3$-SiO_2 (1925), and later he joined Morey & Kracek in a study of the system K_2O-CaO-SiO_2 (1929, 1930).

In the former system the ternary eutectic $Na_2Si_2O_5$-SiO_2-$Na_2O.3CaO.6SiO_2$ is at a temperature of 725°C and commercial soda-lime glasses are of such composition that their crystallization is dominated by this eutectic; that is, they are near or in the stability field of the compound $Na_2O.3CaO.6SiO_2$ (known later as devitrite).

This compound is characterized by an inherent reluctance to crystallize, and its field is one of low temperature and high viscosity. These are the dominant factors in making possible the manufacture of glass, causing it to be above its freezing point at the temperature at which it is manufactured and worked and at its freezing point to be too viscous to crystallize.

This work filled a remarkable gap in knowledge of the melting relations in the common glass system.

Bowen had an enduring interest in the problems offered by the monomineralic rocks. He had, as already remarked, made a field study of anorthosites, and in connexion with his investigations of the mafic monomineralic rocks, particularly those of olivine, he made two visits overseas to the island of Skye in 1923 and 1926. He was fortunate in having as guide Alfred Harker who demonstrated the Tertiary igneous succession of the island and introduced him to the later peridotite dykes which traverse the Cuillin gabbro.

These dykes in their richness in olivine offered a problem and Bowen returned to continue their study in Washington. The outcome of this work enabled him to present a new interpretation of the intrusion mechanism of these ultrabasics and to indicate generally the limit of composition among olivine basalts for existence as original liquids.

The spring of 1927 found Bowen at Princeton delivering a course of lectures on petrogenesis to advanced students in geology.

The substance of these was published in book form in 1928 under the title *The evolution of the igneous rocks.* In the first part of the book were considered aspects of fractional crystallization of magmas in which equilibrium diagrams determined in the laboratory were treated as susceptible of fairly direct application to the natural problems; the second part devoted attention to problems in which the amount of extrapolation from ascertained facts was relatively great or where the diagrams devised were hypothetical—and the topics treated included alkali rocks, lamprophyres, the role of volatiles, petrogenesis and physics of the earth. In this vigorous presentation of the problems of the igneous rocks, Bowen provided a survey and synthesis which has exerted a profound influence on petrologic thought. Long out of print, a reprint edition with a Bowen bibliography and a new introduction by his colleague J. F. Schairer, appeared in 1956.

There now followed a programme of experimental studies in which he was joined by J. F. Schairer on silicate systems containing iron oxide beginning with ferric oxide and later ferrous oxide. This programme carried through from 1929 to 1938 in a series of memoirs of great petrologic interest.

The fusion relations of acmite (1929) and the study of the system Na_2SiO_3-Fe_2O_3-SiO_2(1930) were the first products. The problem of systems engaging the chameleon ferrous oxide brought a particular difficulty to the experimental work. For many years investigation of such systems had proved intractable owing to the difficulties of maintaining them in the ferrous state. In the early studies on ferrous silicates the use of a vacuum furnace had proved of no avail, for even with the low values of oxygen pressure thereby attained a large proportion of the iron of the silicate melt remains in the ferric state.

The use of a reducing atmosphere on the other hand was not regarded favourably owing to the danger of reduction of indefinite proportions of iron to the metallic state. A new attempt to solve the difficulties, involving the use of an iron crucible to hold the charge and a neutral atmosphere (nitrogen) with the expectation that the metallic iron would keep the iron of the melt in the ferrous state, was entirely rewarding. The systems studied following the solution of this difficulty of long standing included FeO-SiO_2 (1932), Ca_2SiO_4-Fe_2SiO_4 (1933) and the ternary systems CaO-FeO-SiO_2 (1934), MgO-FeO-SiO_2 (1935) and finally the system nepheline-silica-fayalite (1938). The results achieved were not only of the first importance to petrologic studies, widening our knowledge of the behaviour of iron rich mafic minerals, olivines and pyroxenes in melts, but were also fundamental for metallurgical slag technology.

In the summer of 1929 Bowen attended the International Geological Congress in South Africa, participating in excursions after the session to localities of special interest in igneous geology, particularly the Bushveld. Opportunity had been arranged for Bowen to undertake a study of the volcanic districts of the Rift-Valleys of Africa, where he accompanied Bailey Willis engaged on a structural survey.

This reconnaissance took them northward through Central Africa along the course of the Western Rift where Bowen studied in particular the small group of Mufumbira volcanoes of the Belgian Congo, climbing the volcano Namlagira.

Turning eastward he examined the Eastern Rift and particularly the volcanic rocks of Kenya, making considerable collections for later laboratory study. The results of these field studies appeared in part much later (1937, 1938) and the work itself was bound up with Bowen & Schairer's prolonged experimental study of the alkali-alumina silicate system ($NaAlSiO_4$-$KAlSiO_4$-SiO_2).

Study on the system had been in progress since 1929, and it was up to this time probably the most difficult and at the same time the most interesting of any studied at the Laboratory. It involved a ternary as well as a binary solid solution series (nephelines, alkali feldspars) of theoretical as well as of practical interest.

A preliminary report on the system appeared in 1935. This defined the primary phase fields, but contained no data correlating compositions of coexisting solid solutions, such were the difficulties of crystallization imposed by viscosity of the melts.

The progress of these studies in the alkali-alumina silicates and on the iron systems prompted an examination of equilibria in systems containing both.

Apart from the intrinsic interest of such compositions, a principal reason for the study was the practical one that iron silicates form liquids of low viscosity and their addition to alkali-alumina silicates endows the complex mixtures in some measure with these desirable attributes conspicuously lacking in alkali-alumina silicates themselves.

Such a study was that of Bowen & Schairer on the system nepheline-albite-silica with fayalite, with its two ternary eutectics (Ab, Fa, Tr) and (Ab, Ne, Fa) showing analogy with natural 'soda rhyolites' and 'soda phonolites' and pointing to fractional crystallization as a dominant control in their development as of the residual liquids in the experimental melts.

A synthesis of the experimental results accumulating on the alkali-alumina silicate systems with lime (anorthite, diopside), and with ferrous oxide (fayalite) in their application to igneous geology formed the subject of an address at the Harvard tercentenary celebrations where Bowen was the recipient of an honorary degree.

It gave strong emphasis to the view that fractional crystallization gave residual liquids enriched in the components of the alkali-alumina silicate system, and it was at this time that Bowen traced out a well defined valley in its fusion surface in which were concentrated the compositions of liquids in essence that were characteristic of rhyolites, trachytes and phonolites, such as he had himself studied among the East African volcanic fields. These compositions Bowen referred to as part of petrogeny's residua system and he concluded that crystal $\rightleftarrows$ liquid equilibrium had been the dominant control in their production as residual liquids of crystallizing magma, as indeed he had no reason to doubt, also of their deep-seated representatives, granites, syenites and nepheline syenites.

Bowen's association with the Geophysical Laboratory had, with the exception of the short term at Queen's in 1919-20, covered a period of 25 years (1912-1937). He was now to make a move.

In this year (1937) he was attracted to Chicago to accept the Charles L. Hutchinson distinguished service professorship of petrology following the retirement of Johannsen from the Department of Geology. Here he was to remain nine years. He rapidly developed a school of experimental petrology, the absence of which in University geological departments he had long deplored, and a succession of papers by his pupils appeared in the next few years dealing with equilibrium studies of alkali systems particularly those containing $NaAlSiO_4$ with gehlenite, wollastonite, albite, anorthite and Na_2SiO_3, that is, selected portions of the quaternary system Na_2O-CaO-Al_2O_3-SiO_2.

He himself was to present a synthesis of these results in their bearing on the origin and differentiation of alkali igneous rocks (1945)—in which he demonstrated the capacity of fractional crystallization to derive from a simplified melilite nephelinite a suite of liquids of alkali character ranging through simplified tephrites to phonolites, and he indicated the possible parallel development of some natural analogues.

This paper appeared as a contribution to the Daly volume of the *American Journal of Science*.

The fruits of the move to Chicago were, however, more quickly revealed—as a product of his attention to problems which developed during his preparation of a course of lectures on metamorphism. His paper on the 'Progressive metamorphism of siliceous limestone and dolomite' (1940) revealed Bowen's resourceful attack in a new field—for this proved a masterly study of the progressive temperature metamorphism of siliceous carbonate sediments of a wide composition range. His recognition and evaluation of the graded series of mineral assemblages derived by the progressive decarbonation of such important geological materials have brought law and order into a region which had hitherto defied systematic treatment. In a concluding commentary on the mineral facies concept he suggested that the PT diagram upon which would eventually be plotted the curves of univariant equilibrium appropriate to a given bulk composition of rock might serve as a petrogenetic grid.

It has already been remarked that a long series of studies on alkali systems had been accumulating over the years 1929 to 1941. The phase equilibrium relations at or slightly below temperatures where a liquid phase was present had gradually been mapped out by Schairer & Bowen in the two ternary systems K_2O-Al_2O_3-SiO_2 and Na_2O-Al_2O_3-SiO_2.

The final diagrams of these systems were published in 1947, but it was not till the close of 1955 and early 1956 that the data on which these diagrams were based were finally made available together with a discussion of some of their petrological and technological applications.

After the second World War when the Geophysical Laboratory was again released from its wartime commitments, the problem of the fields of investigation for the ensuing years was examined anew.

It was clear that a high priority project should be the study of mineral systems embracing volatiles. Pioneer studies had already been made in the Laboratory.

Morey & Fenner had carried out a classic investigation on a silicate system with water in 1917 (H_2O-K_2SiO_3-SiO_2) and Goranson had made notable determinations on the solubility of water in systems approaching granite in composition (1931-1938).

During and immediately after the war, the great interest in certain crystalline phases, especially quartz, had stimulated a great deal of hydrothermal work, and radically new equipment was developing. The time, therefore, appeared ripe for a systematic attack on those problems and so a newly integrated programme of research was outlined in 1946 at the Laboratory.

The programme placed primary emphasis on the study of the melting and solubility relations of silicates in the presence of water and other volatile components under pressure, to obtain a more complete understanding of the genesis of rocks.

There was a strong move to induce Bowen to return once more to the Laboratory to co-operate in this research project. The pressure was successful and Bowen was re-appointed to the staff at the beginning of 1947. Thus was initiated the wet mineral silicate programme of the Laboratory which continues today as a major project.

In the researches that succeeded Bowen was joined by O. F. Tuttle whose experimental skill was to provide in the first place the hydrothermal quenching apparatus which played a vital part in their first joint investigation.

The problem for study was shrewdly chosen—the behaviour of the system-MgO-SiO_2 in the presence of water, results being eventually presented under conditions of temperature up to 1000°C, and water vapour pressures up to 40000 Lb./in^2 (1949). This study opened the new era of the conquest of the wet mineral silicate and in many respects laid the groundwork for much of the later investigations in the hydrothermal field.

One of the important results of the investigation itself was the demonstration that no liquid appeared in the system at the pressures used, even at 1000°C, and there seemed no escape from the conclusion that an olivine or serpentine ultramafic could only be intruded in the solid state.

If these advances shed new light on the peridotite–serpentine enigma, it is clear, as indeed Bowen recognized, that some of the mystery still remained. From these studies Bowen & Tuttle passed to the problems of the feldspar minerals (1950).

Köhler had recognized in 1941 the existence of feldspars exhibiting high- and low-temperature optics. These observations were given a new importance in the experimental study now reported.

Synthetic albite was established as a high-temperature form distinct from the albite of pegmatites and its X-ray and optical properties discriminated.

It was shown further that an inversion relation extended into the plagioclase series and that the high temperature plagioclases were common as phenocrysts in extrusive but not in plutonic rocks.

The liquidus relations in the system $NaAlSi_3O_8$-$KAlSi_3O_8$-H_2O were studied at several pressures of water and here for the first time liquidus determinations were made on compositions closely approaching the natural rocks—the trachytes.

The outstanding feature of the study was the evidence found to support the indications that a complete series of solid solutions exists between potash and soda feldspars at elevated temperatures, but that in the sub-solidus region a solvus exists with two feldspars forming side by side. Homogeneous synthetic feldspars thus exsolve at lower temperatures in confirmation of the accepted theory of most natural perthites.

These investigations were later extended into the system $NaAlSi_3O_8$-$KAlSi_3O_8$-SiO_2-H_2O at water vapour pressures up to 4000 Kg/cm^2 (1952). In the ternary diagram (Or-Ab-SiO_2) Bowen & Tuttle found for the liquidus a thermal trough near the centre towards which all liquids move on crystallization, and this sink is the goal towards which all crystallization courses in the system lead.

The fact that granites have compositions grouped near this minimum was held a strong support for Bowen's long held view of the magmatic origin of granite.

Only a fraction of the experimental data on the 'granite system' has as yet been published, partly in later *Annual Reports of the Director of the Geophysical Laboratory* (1951-1953), in an address by Bowen delivered at Philadelphia (1954) and in a paper by Tuttle (1955). Their collected researches on the origin of granite in the light of these experimental studies are due to appear as a forthcoming memoir of the Geological Society of America. The studies had been initiated at about the time of rising controversy on the origin of granite, and it was at this period that Bowen as the leading magmatist vigorously joined issue in these disputations with the diffusionists. His two notable addresses as President of the Geological Society of America (1946) and as participant in the symposium on the origin of granite at Ottawa (1947), scintillate with humour and testify to his powers as a critic in this continuing warfare.

Long after when the studies on the 'granite system' referred to above were completed, he could reiterate, we may be sure, not without relish, that 'in their extreme clustering about the composition of the liquid of minimum crystallizing temperature, the vast majority of granites loudly proclaim their origin through crystallization of a liquid. The diffusionists may have the small proportion of aberrant granites—tentatively' (1954).

The publication of Bowen's delightfully written paper—'The broader story of magmatic differentiation, briefly told' (1933)—showed how his interests extended into the field of ore deposits, and he repeatedly urged the desirability of equilibrium studies of their mineral phases and of hydrothermal solutions, not only because of the importance of these studies in ore deposition but also because of their significance to many other branches of geology.

It has been well said that phase equilibrium, the keystone of Bowen's structure, enabled him to see the interrelation of geological processes in earth, sea and air, and the importance of extending experimental work over the whole field to bring all into one unified picture.

His philosophy is eloquently expressed in the appeal he made for studies of phase equilibrium before the Geological Society of America special Committee on a possible programme of research in geology (1938).

His own work provided the highest standard of achievement in the great border-land joining geology and chemistry. Though his chief labour remained experimental laboratory research, this he blended continuously with field investigation.

He carried indeed the field to the laboratory in setting up his problems, and in turn, his experimental results to the field in checking them with the rocks.

Some of the products of this dual study have been exemplified in the researches that have been already dwelt on in this appreciation.

In the history of experimental research in petrology, in particular the study of heterogeneous equilibria in silicate melts, he was the great pioneer, in fact his position is unique, for no one has broken so much new ground, nor contributed in such rich measure to the solution of fundamental problems of petrogenesis.

Bowen's association with the Geophysical Laboratory extended in all over thirty-five years. It was a long and splendid record. He was indeed perfectly suited to a lifetime of work in a research institution. Quiet and retiring in disposition, he enjoyed the sheltered environment such an institution can offer and in which he could spend his days almost uninterruptedly on the research he loved.

His two relatively short excursions into University life were, we may be sure, never quite satisfying, and it seemed he had little liking for teaching or administration. His inspiration for research came from his own well ordered mind, his persistent curiosity and his contacts with his immediate colleagues, and he remained as alert in his scientific outlook on his retirement as he had been in his youth.

He was an inspiring influence in the life of the Laboratory, friendly and unassuming, ever ready to help his colleagues from his own deep fund of knowledge. He in his turn owed much, as he recognized, to the loyal support of younger colleagues who joined him in the promotion and execution of long-term researches, and the names of Schairer and Tuttle take pride of place on the list of co-authors in these investigations.

He retired from his position as petrologist at the Laboratory in 1952 at the age of sixty-five.

The occasion was signalized by the publication of a volume dedicated to him—the Bowen Volume—containing contributions from his colleagues and those who had been visitors at the Geophysical Laboratory in past years. After his retirement he moved to Clearwater, Florida, but a year later returned to Washington.

In spite of poor health he was still active and had an office in the Geophysical Laboratory as Research Associate until his death.

He leaves his widow, Mary Lamont Bowen of Washington D.C., and a daughter Catherine (Mrs Jerrold Orne) of Montgomery, Alabama.

Bowen's great achievements were widely recognized both at home and abroad. He served as President of the Mineralogical Society of America in 1937, and of the Geological Society of America in 1946. He had been elected a member of the National Academy of Sciences in 1935.

Among other distinctions were the awards of the Penrose Medal of the Geological Society of America (1941), the Miller Medal of the Royal Society of Canada (1943), the Roebling Medal of the Mineralogical Society of America (1950), the Hayden Medal of the Academy of Natural Sciences of Philadelphia (1953) and the Bakhuis Roozeboom Medal of the Royal Netherlands Academy (1954).

He was honoured abroad by election to honorary membership of numerous societies. British geology had early recognized his achievements in the award by the Geological Society of London of the Bigsby Medal (1931); later he became a Foreign Member of the Society (1937) and received its highest award, the Wollaston Medal in 1950.

The Royal Society elected him a Foreign Member in 1949.

In the preparation of this account I am indebted for much help from colleagues of Bowen at the Geophysical Laboratory. Dr J. W. Greig supplied much information on Bowen's early days in Ontario and Dr J. F. Schairer allowed me to see the biographical account of Bowen he had already prepared. Professors M. B. Baker and J. E. Hawley of Queen's University have also provided me with additional data on Bowen's period at Kingston.

C. E. TILLEY

BIBLIOGRAPHY

(A selected list)

1910. Diabase and granophyre of the Gowganda Lake District, Ontario. *J. Geol.* **18**, 658-674.
1912. The composition of nephelite. *Amer. J. Sci.* **33**, 49-54.
1912. The order of crystallization in igneous rocks. *J. Geol.* **20**, 457-468.
1912. The binary system: $Na_2Al_2Si_2O_8$ (nephelite, carnegieite)—$CaAl_2Si_2O_8$ (Anorthite). *Amer. J. Sci.* **33**, 551-573.
1913. The melting phenomena of the plagioclase feldspars. *Amer. J. Sci.* **35**, 577-599.
1913. The order of crystallization in igneous rocks. *J. Geol.* **21**, 399-401.
1914. (With Olaf Andersen.) The binary system MgO-SiO_2. *Amer. J. Sci.* **37**, 487-500.
1914. The ternary system: diopside—forsterite—silica. *Amer. J. Sci.* **38**, 207-264.
1915. Crystallization-differentiation in silicate liquids. *Amer. J. Sci.* **39**, 175-191.
1915. The crystallization of haplobasaltic, haplodioritic and related magmas. *Amer. J. Sci.* **40**, 161-185.
1915. The later stages of the evolution of the igneous rocks. *J. Geol.* **23**, Suppl., 1-89.
1917. The sodium potassium nephelites. *Amer. J. Sci.* **43**, 115-132.
1917. The problem of the anorthosites. *J. Geol.* **25**, 209-243.
1918. The significance of glass-making processes to the petrologist. *J. Wash. Acad. Sci.* **8**, 88-93.
1919. Devitrification of glass. *J. Amer. Ceram. Soc.* **2**, 261-278.
1919. Abnormal birefringence of torbernite. *Amer. J. Sci.* **48**, 195-198.
1919. Crystallization-differentiation in igneous magmas. *J. Geol.* **27**, 393-430.
1920. Differentiation by deformation. *Proc. Nat. Acad. Sci., Wash.* **6**, 159-162.
1921. Diffusion in silicate melts. *J. Geol.* **29**, 295-317.
1922. Genetic features of alnoitic rocks from Isle Cadieux, Quebec. *Amer. J. Sci.* **3**, 1-34.
1922. The reaction principle in petrogenesis. *J. Geol.* **30**, 177-198.
1922. (With G. W. Morey.) The melting of potash feldspar. *Amer. J. Sci.* **4**, 1-22.
1922. The behaviour of inclusions in igneous magmas. *J. Geol.* **30**, 513-570.
1923. The genesis of melilite. *J. Wash. Acad. Sci.* **13**, 1-4.
1923. (With M. Aurousseau.) Fusion of sedimentary rocks in drill holes. *Bull. Geol. Soc. Amer.* **34**, 431-448.
1924. (With J. W. Greig.) The system: Al_2O_3-SiO_2. *J. Amer. Ceram. Soc.* **7**, 238-254.
1924. (With J. W. Greig and E. G. Zies.) Mullite, a silicate of alumina. *J. Wash. Acad. Sci.* **14**, 183-191.
1924. The Fen area in Telemark, Norway. *Amer. J. Sci.* **8**, 1-11.
1924. (With G. W. Morey.) The binary system sodium metasilicate-silica. *J. Phys. Chem.* **28**, 1167-1179.
1925. The mineralogical phase rule. *J. Wash. Acad. Sci.* **15**, 280-284.
1925. (With J. W. Greig.) The crystalline modifications of $NaAlSiO_4$. *Amer. J. Sci.* **10**, 204-212.
1925. (With G. W. Morey.) The ternary system sodium metasilicate calcium metasilicate-silica. *J. Soc. Glass Tech.* **9**, 226-264.
1926. Properties of ammonium nitrate: I A metastable inversion in ammonium nitrate; II The system: ammonium nitrate-ammonium chloride; III A note on the system ammonium nitrate-ammonium sulphate. *J. Phys. Chem.* **30**, 721-737.
1926. (With R. W. G. Wyckoff and J. W. Greig.) The X-ray diffraction patterns of mullite and sillimanite. *Amer. J. Sci.* **11**, 459-472.
1926. The carbonate rocks of the Fen area in Norway. *Amer. J. Sci.* **12**, 499-502.
1927. The origin of ultrabasic and related rocks. *Amer. J. Sci.* **14**, 89-108.

1928. *The evolution of the igneous rocks.* Princeton University Press, Princeton, New Jersey, x + 334 pp.

1928. Geologic thermometry. In *The laboratory investigation of ores* edited by E. E. Fairbanks. McGraw-Hill, New York, Chapter 10, pp. 172-199.

1929. (With J. F. Schairer.) The system: leucite-diopside. *Amer. J. Sci.* **18**, 301-312.

1929. (With J. F. Schairer.) The fusion relations of acmite. *Amer. J. Sci.* **18**, 365-374.

1929. (With F. C. Kracek and G. W. Morey.) The system potassium metasilicate-silica. *J. Phys. Chem.* **33**, 1857-1879.

1930. Central African volcanos in 1929. *Trans. Amer. Geophys. Union 10th and 11th Annual Meetings*, pp. 301-307. Nat. Res. Council, Washington D.C.

1930. (With G. W. Morey and F. C. Kracek.) The ternary system K_2O-CaO-SiO_2 (with correction). *J. Soc. Glass Tech.* **14**, 149-187.

1930. (With J. F. Schairer and H. W. V. Willems.) The ternary system. Na_2SiO_3-Fe_2O_3-SiO_2. *Amer. J. Sci.* **20**, 405-455.

1931. (With E. Posnjak.) The role of water in tremolite. *Amer. J. Sci.* **22**, 203-214.

1932. (With J. F. Schairer.) The system FeO-SiO_2. *Amer. J. Sci.* **24**, 177-213.

1933. Vogtite, isomorphous with wollastonite. *J. Wash. Acad. Sci.* **23**, 87-94.

1933. (With J. F. Schairer and E. Posnjak.) The system Ca_2SiO_4-Fe_2SiO_4. *Amer J. Sci.* **25**, 273-297.

1933. The broader story of magmatic differentiation, briefly told. In *Ore deposits of the Western States*, Amer. Inst. Min. Met. Eng., New York, Chapter III, Pt. II, pp. 106-128.

1933. (With J. F. Schairer and E. Posnjak.) The system CaO-FeO-SiO_2. *Amer. J. Sci.* **26**, 193-284.

1935. (With J. F. Schairer.) The system MgO-FeO-SiO_2. *Amer. J. Sci.* **29**, 151-217.

1935. (With J. F. Schairer.) Preliminary report on equilibrium-relations between feldspathoids, alkali-feldspars and silica. *Trans. Amer. Geophys. Union, 16th Annual Meeting*, pp. 325-328. Nat. Res. Council, Washington, D.C.

1935 (With J. F. Schairer.) Grünerite from Rockport, Massachusetts, and a series of synthetic fluor-amphiboles. *Amer. Mineral.* **20**, 543-551.

1935. (With J. F. Schairer.) The problem of the intrusion of dunite in the light of the olivine diagram. *Rept. XVI Internat. Geol. Congress*, pp. 391-396, Washington, D.C.

1935. 'Ferrosilite' as a natural mineral. *Amer. J. Sci.* **30**, 481-494.

1936. (With R. B. Ellestad.) Nepheline contrasts. *Amer. Mineral.* **21**, 363-368.

1936. (With J. F. Schairer.) The system albite-fayalite. *Proc. Nat. Acad. Sci. Wash.* **22**, 345-350.

1937. Recent high-temperature research on silicates and its significance in igneous geology. *Amer. J. Sci.* **33**, 1-21.

1937. (With R. B. Ellestad.) Leucite and pseudoleucite. *Amer. Mineral.* **22**, 409-415.

1938. (With J. F. Schairer.) Crystallization equilibrium in nepheline-albite-silica mixtures with fayalite. *J. Geol.* **46**, 397-411.

1938. Lavas of the African Rift Valleys and their tectonic setting. *Amer. J. Sci.* **35**-A, 19-33.

1938. (With J. F. Schairer.) The system, leucite-diopside-silica. *Amer. J. Sci.* **35**-A, 289-309.

1938. 'Mente et malleo atque catino.' Presidential address 18th Annual Meeting, Mineralogical Society of America. *Amer. Mineral.* **23**, 123-130.

1939. Geology and chemistry. *Science*, **89**, 135-139.

1940. Progressive metamorphism of siliceous limestone and dolomite. *J. Geol.* **48**, 225-274.

1940. Geologic temperature recorders. *Sci. Monthly*, **51**, 5-14.

1941. Certain singular points on crystallization curves of solid solutions. *Proc. Nat. Acad. Sci. Wash.* **27**, 301-309.

1941. Physical controls in adjustments of the earth's crust. In *Shiftings of the seafloors and coastlines*, Univ. Penn. Bicentennial Conference. Univ. Penn. Press, Phila, pp. 1-6.

1942. (With J. F. Schairer.) The binary system $CaSiO_3$-diopside and the relations between $CaSiO_3$ and akermanite. *Amer. J. Sci.* **240**, 725-742.

1943. Petrology and silicate technology. *J. Amer. Ceram. Soc.* **26**, 285-301.

1945. Phase equilibrium bearing on the origin and differentiation of alkaline rocks. *Amer. J. Sci.* **243**-A, 75-89.

1947. Magmas. *Bull. Geol. Soc. Amer.* **58**, 263-280.

1947. (With J. F. Schairer.) The system anorthite-leucite-silica. *Bull. Soc. Geol. Finlande*, **20**, 67-87.

1948. The granite problem and the method of multiple prejudices. *Geol. Soc. Amer. Mem.* **28**, 79-90.

1949. (With O. F. Tuttle.) The system MgO-SiO_2-H_2O. *Bull. Geol. Soc. Amer.* **60**, 439-460.

1950. (With O. F. Tuttle.) The system $NaAlSi_3O_8$-$KAlSi_3O_8$-H_2O. *J. Geol.* **58**, 489-511.

1950. (With O. F. Tuttle.) High-temperature albite and contiguous feldspars. *J. Geol.* **58**, 572-583.

1950. The making of a magmatist. *Amer. Mineral.* **35**, 651-658.

1954. Experiment as an aid to the understanding of the natural world. *Proc. Acad. Nat. Sci. Philad.* **106**, 1-12.

1955. (With J. F. Schairer.) The system K_2O-Al_2O_3-SiO_2. *Amer. J. Sci.* **253**, 681-746.

1956. (With J. F. Schairer.) The system Na_2O-Al_2O-Si_3O_2. *Amer. J. Sci.* **254**, 129-195.

1956. *The evolution of the igneous rocks.* (Reprint of 1928 volume with introduction by J. F. Schairer and bibliography of the published works of the author.) Dover Publications, Inc. New York, xviii + 334 pp.

1957. (With O. F. Tuttle.) The origin of granite in the light of experimental studies in the system $NaAlSi_3O_8$-$KAlSi_3O_8$-SiO_2-H_2O. *Geol. Soc. Amer. Mem.* (in press).

H. Bowman

FREDERICK GEORGE DONNAN

1870-1956

His life

This biography is compiled from the Personal Record which Donnan left with the Royal Society, the assistance of numerous pupils and friends, and from the recollections of the writer extending over a long period.

Donnan was born on 5 September 1870 at Colombo, Ceylon, and was the second of a family consisting of two brothers and four sisters. His father was William Donnan, a merchant of Belfast, and his mother's maiden name was Jane Ross Turnley Liggate she also being a native of Northern Ireland.

Donnan's uncle, Captain James Donnan, was 'master attendant', Colombo and Inspector of the Ceylon Pearl Fisheries, who received a C.M.G. on his retirement; his son, Donnan's cousin, was a regular soldier who spent most of his life in India, retiring with the rank of Colonel in 1913, rejoining the Army in 1914, and dying of heat stroke in Mesopotamia.

Donnan's brother, William Dunlop Donnan, was a general medical practitioner (M.D. of the Queens University) of Belfast till his death in 1941.

Donnan returning from Ceylon at the age of 3 had no recollections of that country at all and this must be considered in making any appreciation of him as a devoted Ulster patriot.

Donnan was a bachelor, so particular and generous mention must be made of his two sisters Jane and Leonora (Nora) who played such an important and unobtrusive part in his life. Jane was his secretary at University College and later looked after the I.C.I. Research Associates working there; after his retirement she carried on with his correspondence. In these later years her eyesight began to fail and steadily worsened. She died only three days before her brother and they were cremated on the same day. On duty she was most intelligent and efficient and in private life had all the family charm.

Leonora (Nora) kept house for Donnan for about 38 years, dying in May 1956. Donnan's father who spent most of his working life in the firm of Brown Corbett & Co. of Belfast, was in his earlier years in the firm of Shand Brothers, Ceylon.

Donnan lived with his parents near Bangor, County Down, from 1875 to 1879, and in Holywood in the same County from 1884 to 1893, in the intervening period they lived in Belfast.

In the year 1879 he lost his left eye. I knew Donnan intimately for more than fifty years and to the best of my recollection he only mentioned it once. His mother was a most remarkable, kindly, intelligent old lady who retained all these qualities including her good looks till she was very well over eighty and she told me exactly how it happened.

Donnan and another little boy were playing a kind of hide and seek in a row of unfinished houses, creeping up and down flights of stairs throwing mortar at each other. A handful of the mortar which unfortunately was freshly mixed caught Donnan fairly in the face. By the time they got him to hospital an instant operation to remove his left eye was necessary and for several days there were grave fears for the sight of the other eye.

When he returned home he went into the nursery and removed the left eyes of all his sisters' dolls.

This disability did not prevent him from playing quite a reasonable game of lawn tennis. At school he was a member of the Rugby 1st XV and the school lacrosse team.

He was a first class swimmer and high diver, sometimes diving from as high as 30 ft.

He attended the Belfast Royal Academy from 1880 to 1889, the then head master Dr W. F. Collier was an excellent teacher from whom he learned much concerning the literature and history of England. As a schoolboy he was chiefly interested in mathematics and physical science. At the annual examinations of the Irish schools conducted by the Intermediate Education Board he obtained first class 'Exhibitions' in 1886, 1887 and 1888. As the total value of these exhibitions was about £100 he was able to defray a considerable part of the cost of his school education.

There were, however, no scientific laboratories in the Belfast Royal Academy, so although he studied some physics and chemistry there, the instruction was purely theoretical. He managed to get some practical training by attending evening courses conducted by a Mr Adam Speers, of the Sullivan Schools at Holywood. After the examination he was awarded a silver medal.

The course was under the aegis of the science and art department and Donnan always acknowledged his debt to Mr Speers, who was a man with a great enthusiasm for chemistry and from whom he derived a good deal of inspiration.

Donnan entered Queen's College, Belfast in 1889 and remained there till 1893. His record shows his remarkable industry and ability, indeed as his old mother told me 'after the accident to his eye he never looked back'. He was awarded a science scholarship in 1890, a senior scholarship in 1892 and the Andrews studentship in the same year.

At the annual examinations of the Royal University of Ireland he obtained the following distinctions. In 1890, a First Class Exhibition and the first place in Ireland, repeating this feat in 1891. In 1892 he was awarded a First Class Exhibition and first place in chemistry and physics.

At the B.A. examination in 1894 he got first place and a studentship (value

£300) in chemistry and physics. In the M.A. examination in 1897 he was awarded a Junior Fellowship (£400 p.a. for 4 years) as the result of an examination in chemistry and physics. As a Junior Fellow he acted as Assistant Examiner for 4 years. He studied chemistry, mathematics and physics under Professors Letts, Purser and Everitt. He also worked at English literature, Latin, logic and philosophy. He always spoke in terms of the highest admiration of Professor Purser who gave them all a love for pure mathematics.

In 1893 Donnan was awarded an 1851 Exhibition Research Scholarship and with this he went to the University of Leipzig and did a year's chemistry under Wislicenus. He then changed over to the laboratory of Wilhelm Ostwald in the same University in order to devote himself to the young and rising discipline of physical chemistry. In 1896 he was awarded a Ph.D. degree (magna cum laude) for a thesis on the experimental study of the relation between ionization and light absorption in an aqueous solution of violuric acid either alone or mixed with colourless acids. To do this he designed and constructed a new type of colorimeter familiar to many as the Donnan colorimeter. In 1896-97 he worked in the laboratory of J. H. van 't Hoff in Berlin. Here he studied experimentally the hydrates of calcium sulphate and the vapour pressures of a number of saturated aqueous solutions of single and double salts occurring in van 't Hoff's investigations on the oceanic salt deposits.

It has already been mentioned that he had gained a junior fellowship in 1897. He, therefore, decided to spend the year 1897-98 quietly at home in order, as he said, 'to read deeply into the literature of physical chemistry'. During this period he published four theoretical papers, namely, (1) 'On Lord Rayleigh's proof of van 't Hoff's osmotic theorem', (2) 'The Thompson effect on a binary electrolyte', (3) 'On the Hall effect in a binary electrolyte' and (4) 'On the isothermal pressure surface in the case of two single salts and one double salt'.

In 1898 Donnan went as a senior Research Student in Ramsay's Laboratory at University College London, and in the period 1899-1901 published experimental researches on the nature of soap solutions, the relative rates of effusion of argon, helium and other gases. Also two theoretical papers on the phenomenon of colloidal solution.

It was not until 1901 that he obtained his first post, his post-graduate study having lasted from 1893 until 1901 and having carried him to Leipzig, Berlin and London; then in his 31st year he became an assistant lecturer in Ramsay's Laboratory at a salary of £100 per annum. His 1851 exhibition and his junior fellowship had kept him going as a 'free researcher', as he said, sometimes he wondered if he did not delay too long before entering professional academic life. My own opinion is that having regard to the fact that at the age of 85 at least his mind was undimmed and also to the distinguished men with whom he worked in his younger days, he could not have done better.

He was made Assistant Professor in University College in 1902 and remained in this position until 1903, when he became Lecturer on Organic Chemistry at the Royal College of Science, Dublin. In the year 1904, however, a new Chair of Physical Chemistry was formed in the University of Liverpool by Sir John Brunner and Donnan was invited to be its first occupant. He supervised the building of the Muspratt Laboratory of Physical Chemistry at Liverpool and was its director from 1906 to 1913, when he succeeded Sir William Ramsay as Professor of General Chemistry and held this Chair and the Directorate of the Chemical Laboratories in U.C.L. until his retirement in 1937.

He was elected a Fellow of the Royal Society in 1911.

During the 1914-18 war Donnan was in the thick of the scientific and technological battle, being very closely associated with Lord Justice Fletcher Moulton, F.R.S., the Director General of Explosives Supply. The rather dramatic manner in which it began is worth recording.

In 1915 the writer and his colleagues in Brunner Mond & Co., at Northwich in Cheshire, were engaged in a desperate and ultimately very successful attempt to manufacture large quantities of high explosives by what can only be called stop-gap methods. One of these was the production of ammonium nitrate from sodium nitrate and ammonium sulphate. By almost a miracle this and similar processes had been worked out in principle before the war, that is to say the formal hetergeneous equilibria had all been determined. The process under discussion offered, however, peculiar difficulties in getting to work on the large scale. There were a large number of double salts involved and our first attempt was unsuccessful. A further close study of the equilibria made it pretty certain that it would work. Moulton told me that as he was going to spend a great deal of money he must have an outside opinion, who would I have? I said Donnan.

Donnan was lecturing, a messenger from Moulton came in who stopped his lecture and took Donnan to see Moulton at once. Donnan came to Northwich, reported favourably on the process and it was a great success. Moulton instantly recognized Donnan's remarkable abilities and formed a firm friendship with him which lasted for the rest of his life, and employed him in a very large number of ways.

Donnan worked closely with K. B. Quinan, Moulton's remarkable dynamic American chemical-engineer, who suffered fools anything but gladly, and who at once took to Donnan who assisted him in the designs of plant for the fixation of nitrogen and for the production of mustard 'gas'. Donnan played an important part in the early stages of the research work on synthetic ammonia and nitric acid working with Dr Harper in the Chemistry Department of University College, London. The strong research team included Greenwood, H. S. Taylor, E. K. Rideal and J. R. Partington, who worked in a special high-pressure laboratory.

At the end of the war, by this time an intimate of the Board of Brunner Mond & Co., he played his part in advising them to develop this work and always took pleasure in the recollection that he was one of the forces which helped to produce the great works at Billingham.

In the 1914 war he was also a member of the Chemical Warfare Committee of the Wartime Inventions Board and of the Nitrogen Products Committee.

For his war work he was awarded the C.B.E.

His connexion with the chemical industry which began in the 1914 war did not cease with it. He was research consultant to Brunner Mond & Co. from 1920 to 1926 and a member of the Research Council of I.C.I. Ltd from 1926 to 1939. He was also Research Consultant to the Royal Dutch Shell Co. from 1936 to 1940.

Donnan was travelling with Allmand in Germany just before the 1914 war, they managed to escape through St Petersburg and Sweden. In 1940 Donnan on a visit to the Shell Co. in Amsterdam again got away just before the German invasion.

The very large number of awards, distinctions and requests to deliver important addresses affords some measure of the esteem in which he was held. He received no less than eleven Honorary Degrees consisting of that of L.L.D. from St Andrews, Edinburgh and Johns Hopkins (Baltimore) and eight Honorary Doctorates of Science from the Universities of Belfast, Liverpool, Princeton, Oberlin, Durham, Athens, Coimbra and the National University of Ireland. He was elected a Fellow of University College London, in 1936. In 1924 the Chemical Society awarded him the Longstaff Medal for chemical research. In 1928 he received the Davy Medal of the Royal Society for his contributions to physical chemistry.

As already mentioned he was elected into the Royal Society in 1911, he served on the Council in the years 1924-26, was Treasurer of the R.S. Club from 1942-44 and Chairman of the Royal Society Scientific Relief Committee from 1941 to 1946.

He was Foreign Secretary of the Chemical Society from 1925 to 1933 and President from 1937 to 1939.

He was a Founder Member (1903) of the Faraday Society and President 1924-26. He was President of the Association of Scientific Workers 1939-40 and President of the British Association of Chemists 1940-41. President of the Society for Visiting Scientists 1943-46.

President of Section B of the British Association for the Advancement of Science in Liverpool in 1923 and a member of the Advisory Council of the Department of Scientific and Industrial Research 1925-30.

He was an Honorary member of the following Societies: The Royal Academy of Science of Amsterdam, The Royal Society of Sciences of Uppsala, The Royal Physiographical Society of Lund, The Royal Society of Edinburgh, The National Institute of Science of India, The Indian Academy of Science, Bangalore, The American Chemical Society, The Royal Netherlands Chemical Society, The Deutsche Bunsen Gesellschaft, The Roumanian Chemical Society, The Austrian Chemical Society, The Institution of Chemical Engineers, the Société de Chemie Industrielle, Paris, The Société

Philomathique Paris, The International Society of Leather Trades Chemists (British Section) and the Faraday Society (1950).

He delivered the Ostwald Memorial Lecture to the Chemical Society in 1933 and the E. J. Cohen Memorial Lecture in 1947 to the same body.

He delivered the Presidential address to Section B of The British Association in 1923 on 'Some aspects of the physical chemistry of interfaces' and an evening address to the B.A., in Glasgow, 1928, on 'The mystery of life'. To the Morley College in London in 1931, he gave their address on 'Chemistry and radiation'.

He gave the Ludwig Mond Memorial Lecture to the Royal Institution and the Royal Institute of Chemistry, and the first Liversedge Lecture to the Chemical Society in 1928 on 'Physical chemistry in the service of biology'. His two Presidential addresses to the Chemical Society were entitled 'Physical retrospect and prospect 1938' and 'The role of osmotic pressure in the development of chemical and biochemical science 1939'.

(1) He was Chairman of the Committee of the British Association which between 1917-23, inclusive, issued five reports on colloid science and its industrial application.

(2) He succeeded Sir William Ramsay as Editor of the *Text books of physical chemistry* (Longman) and was active in the period 1918-38.

(3) He was Editor in the series of scientific monographs published by Herman et Cie, Paris, entitled *Actualites scientifiques* for such as dealt with the colloidal state.

Three memoirs were published—*Thixotropy*, by H. Freundlich, 1938, *The sol gel transformation*, by E. Heymann, 1936, and *Aqueous solutions of paraffin salts*, a study in micelle formation by G. S. Hartley, 1936.

(4) Editor with A. Haas of Vol. 1, *Thermodynamics of the commentary on the scientific writings of J. Willard Gibbs*, published by Yale University Press, New Haven, U.S.A. for the Yale University Committee on the Gibbs commentary.

This very heavy and exacting work took much of his spare time from 1928 to 1936. He was at it night after night until the small hours of the morning. Owing to various causes practically all the work fell on him.

Financial assistance for scientific work

Donnan was instrumental in raising large sums of money from enlightened private persons and from industry. He was particularly fitted to do this from the cogency of his arguments, his skill on stating a case and the number of influential people by whom he was known and well liked.

In 1913 when examining the plans and estimates for the University College Chemical Laboratory then being built and the amount of money which the College had in hand for this purpose. He calculated with the help of his friend

Sir Robert Blair, Chairman of the Higher Education Committee of the L.C.C. that a further sum of £20 000 would be necessary. This sum was voted in July 1914, a fortnight before war broke out, which saved the laboratory in the nick of time. When teaching work in the laboratory in 1915—the war work especially—and much other work was hampered by lack of a workshop, Donnan, knowing Messel very well, put the case to him, estimated it would cost £1000 and got the money at once. This set the workshop speedily going. In 1919 the question of funds for research work became acute not only in University College, but all over the country. Donnan talked it over with Moulton who took him to see Roscoe Brunner the Chairman of Brunner Mond & Co. and also invited Donnan to draw up a scheme of annual financial grants for the Chemistry Departments of the Universities of the U.K. in order to enable them to carry out research in physical and inorganic chemistry. The Company adopted his scheme. Later when I.C.I. was formed Donnan was for many years a member of the I.C.I. Research Council which expanded the scheme to cover organic chemistry as well. Donnan played an important part in this large and enlightened scheme which included not only Universities but some of the larger Technical Colleges.

Donnan, who on account of his American tours was well known and very much liked there, played a large part in persuading the Rockefeller Foundation and other American sources to provide great sums of money for the rebuilding of the Royal Institution between the wars. He also helped Kamerlingh Onnes to obtain money for his laboratory from the U.S.A.

His travels

Donnan travelled extensively and always used his good offices for international co-operation as far as it was in any way possible. In 1920-22 Donnan together with his brilliant Dutch friend, Ernst Cohen, organized the first International Meeting of Chemists after the 1914-18 war. This took place in Utrecht, financed by Dutch industrial corporations.

He was invited to undertake special tours in Holland (1922), U.S.A. 1923-24, Sweden 1924, Denmark 1935, Germany 1932 and 1939.

He was an enthusiastic supporter of the Deutsche Bunsen Gesellschaft, and attended their meetings in Munich 1928, Berlin 1929, Vienna 1931 and Karlsruhe 1933. In Karlsruhe in 1933 he and I made arrangements for people like Simon, London, Teller, Peierls, Freundlich, etc., to come to Great Britain, which they did.

Donnan retired from the Chair at U.C.L. in 1937 and lived on into the war at his old house at 23 Woburn Square. As the bombing grew worse it was obvious that his house was in great danger. He moved his furniture out just in time, the house was destroyed twelve hours later in October 1940. In November 1940 he went to live in Hartlip, Sittingbourne, Kent, where he remained until his death on 16 December 1956.

HIS WORK

Anyone going through Donnan's bibliography will immediately be struck by the very wide range of his interests. For example, his early appreciation of the necessity of a united Europe made him acutely conscious of the difficulties which would arise owing to the language bar, hence his interest in and close acquaintance with the problem of artificial languages. Even in the years before his retirement the shortage of men trained in the natural sciences was becoming manifest, he had views on this. He was also an early enthusiast for the new discipline of chemical engineering which has now come into its own.

After these preliminary remarks we can turn to the more serious side of his work. His very early papers reflect his long and unusual training. It is well to remember that from the classification point of view Donnan was in the zone between physics and physical chemistry. In a private letter to Donnan from Ramsay discussing his chances of election into the Royal Society, Ramsay said that this was a serious difficulty as he was in danger of falling between two stools, things being what they were in those days.

At University College in Ramsay's time Donnan was the only man who was well acquainted with the Arrhenius-van 't Hoff-Ostwald school of physical chemistry and he began to apply these ideas to the problems with which he was surrounded.

In 1900 Ramsay asked him to study the rates of effusion of some of the rare gases. A little later with Baly he worked on the surface energies and densities of a number of liquefied gases with the idea of producing a commercial process for the manufacture of liquid air and oxygen. With Mrs Burke, again at Ramsay's suggestion, he investigated the rates of reaction of the alkyl iodides with silver nitrate and ran into the anomalies caused by the neutral salt effect which he later discussed with Arrhenius.

About this time he worked with le Rossignol on the reaction between potassium ferricyanide and potassium iodide; this they found to be quinquemolecular. He worked with Bassett on the colour of cobalt, iron and copper salts from the standpoint of electro affinity.

He also worked with Burt on the transition temperatures and hydrates.

It could be argued that some of the above work was reminiscent of his career in Germany, but he soon began to strike out for himself. In van 't Hoff's laboratory he had begun some work on the nature of soap solutions which he completed in University College London. He showed that the efficiency of hydroxyl ions in the emulsification of oils depended on the formation of soaps on the surface layer.

Thus neutral olive oil is only slightly emulsified with alkali, but olive oil containing free acid is readily emulsified.

This work was the beginning of a series of papers on colloidal theory which gave him an international reputation as a colloid chemist. His theory of the enhanced stability which an emulsifier gives to an emulsion was developed in the above paper in terms of an increased concentration of the surface agent at the point of contact of two drops. He published papers on a thermodynamic theory of colloidal solutions. He attacked this problem from the standpoint of capillarity alone and discussed the causes for the limiting sizes of colloidal particles in terms of differences between the variations in the molecular forces (A—A; A—B) with distance.

In the last of these papers he made use of an idea suggested to him by van 't Hoff on the possibility of a negative interfacial tension. This was done with his fellow countryman W. C. McC. C. Lewis who, working with him in the early part of his Liverpool period studied electrophoretic effects and tested the applicability of the Gibbs equations, this was done in 1908-09 and 1910.

About this period the building of the Muspratt Laboratories absorbed the greater part of Donnan's energies and slowed down his rate of publication. With Potts he studied the effects of the sodium salts of the straight-chain acids ($n = 1$ to 13) on the oil/water interface. Using a drop pipette he measured the interfacial tensions of the oil/water interface and showed that there was good correlation with the emulsifying power of the salts. There was an optimum concentration for emulsification of each salt and a rapid increase of the effects above C_{12}. The results were in general agreement with the equations of Gibbs.

The applicability of these equations he confirmed with Barker for the air/water interface using nonylic acid and safronine as surface agents. Both the surface tensions and the interface concentrations were experimentally determined and the values agreed in order of magnitude with those calculated from the Gibbs equation.

Membrane equilibria

(Donnan, F. G., 1911. *Z. Elektrochemie* **17**, 572)

Publishing a paper is rather like throwing a stone into water. The ripples sometimes go a very long way. This is particularly true of the paper he published in 1911.

It is sufficiently important to quote in his own words: 'We suppose that the membrane (indicated on the following diagram by a vertical line) be impermeable for the anion R of a salt NaR (and also for the non dissociated part of the salt NaR) but permeable for all the other ions and salts to be considered in this connection. . . .

'Suppose that in the beginning we have a solution of NaR on one side of the membrane (indicated by a vertical line) and of NaCl on the other side

$$\begin{array}{c|c} \overset{+}{\mathrm{Na}} & \overset{+}{\mathrm{Na}} \\ \overset{-}{R} & \overset{-}{\mathrm{Cl}} \\ (\mathrm{I}) & (\mathrm{II}) \end{array}$$

In this case NaCl will diffuse from II to I. In the end the following equilibrium will result

$$\begin{array}{c|c} \overset{+}{\text{Na}} & \overset{+}{\text{Na}} \\ \overset{-}{R} & \\ \overset{-}{\text{Cl}} & \overset{-}{\text{Cl}} \\ \text{I} & \text{II} \end{array}$$

when this equilibrium is established, the energy required to transport reversibly and isothermally 1 gram molecule of Na from II to I equals the energy which can be gained by the corresponding reversible and isothermal transport of a gram molecule of Cl. In other words, we consider the following infinitely small isothermal and reversible change of the system

$$\left\{ \begin{array}{l} (\delta n \text{ mol } \overset{+}{\text{Na}} \text{ (II)} \rightarrow \text{I} \\ (\delta n \text{ mol } \overset{-}{\text{Cl}} \text{ II} \rightarrow \text{I} \end{array} \right\}$$

The energy which can be gained in this way (i.e. the diminution of the free energy) is zero hence

$$\delta n\, RT \log \frac{[\overset{+}{\text{Na}}] \text{ II}}{[\text{Na}] \text{ I}} + \delta n\, RT \log \frac{[\text{Cl}] \text{ II}}{\text{Cl I}} = 0$$

or $[\overset{+}{\text{Na}}] \text{ II} \cdot [\overset{-}{\text{Cl}}] \text{ II} = [\overset{+}{\text{Na}}] \text{ I} \cdot [\text{Cl}]$

where the brackets signify molar concentrations.'

This means that if the concentration of the non dialyzing ion R' in I is high compared with the concentration of Na, Cl in II then the distribution of Cl′ on the two sides of the membrane at equilibrium will be markedly unequal, although the membrane is completely permeable to Cl′. Similar effects should occur for electrolytes without ions in common provided one ion is impermeable.

If there is Na*R* on one side and pure water on the other and H*R* is a weak acid then since Na is freely mobile across the membrane hydrolytic dissociations will occur such that

$$\frac{[\text{Na}^{\cdot}] \text{ I}}{[\text{Na}^{\cdot}] \text{ II}} = \frac{[\text{OH}'] \text{ II}}{[\text{OH}'] \text{ I}}$$

It follows, therefore, that in measurements of the osmotic pressure of a colloidal electrolyte Na*R*, the magnitude of the osmotic pressure will be modified by diffusion of ions across the membrane.

The above conclusions were tested experimentally by Donnan and Harris and completely confirmed for the system where NaR was Congo Red. Other tests of the theory were made where R' was the ferrocyanide ion, the membrane being copper ferrocyanide, agreement with the theory was again found (Allmand 1914, 1919).

Proctor (*J. Chem. Soc.* 1914, **105**, 313) and Proctor and Wilson (*J. Chem. Soc.* 1916, **109**, 307) applied the theory to the swelling of gelatine in electrolytes.

Donnan effects can be obtained even when a membrane as such is not present, provided there is a suitable constraint to the free movement of one of the ions of a colloidal electrolyte. Thus when a piece of protein is placed in hydrochloric acid, the chlorine ion of the protein chloride is free to exert an O.P., but the protein anion is rigidly held in the molecular frame work. According to Proctor and Wilson the swelling of the protein arises as the result of excess of crystalloids inside the gel due to the setting up of a Donnan equilibrium.

This suggestion for the causes of the swelling of gelatine was not accepted immediately, it was not in accord with the current lines of thought about proteins.

The work of Loeb (*Proteins and the theory of colloidal behaviour*, 1922), however, established that proteins were amphoteric electrolytes and that the ionic dissociation of the amino and carboxyl groups obeyed the ordinary laws of mass action. Loeb made measurements of the changes in membrane potentials, osmotic pressure, and viscosities of protein and protein solutions caused by the addition of electrolytes, and found good agreement with the Donnan theory. Loeb's view was summarized in the preface of his book. He said, 'Any rival theory which is intended to replace the Donnan theory must be able to accomplish at least as much as the Donnan theory, i.e. it must give a quantitative, mathematical, and rationalistic explanation of the curves expressing the influence of hydrogen ion concentration, valency of ions and concentration of electrolytes on colloidal behaviour, and it must explain these curves not for one property alone, but for all the properties, electric charges, osmotic pressure, swelling, viscosity, and stability of solutions since all these properties are effected by electrolysis in a similar way.'

The Donnan theory is now part of the framework of colloid chemistry, and provides the basis for many of our ideas of the nature of the transport of ions and molecules across the living cell.

The Donnan theory lights up Donnan, his work and his methods.

All the great variety of his researches is an attempt to fit things into a rational and, if possible, a mathematical framework.

The effect of Moulton's dramatic call during World War I is plainly seen in his bibliography, but what is not so manifest is his intense interest in the newer chemistry and cosmology, to which he devoted a great deal of his leisure, probably more than many people would have done. He gave a great deal of thought to the origin of life itself.

He worked at mathematical physics up to the last. He once said to me 'Freeth I am sure you must have Irish ancestry' I said 'Well it must be a long way back, but why?' He said 'An Irishman is a man who works like the devil at things which are not really his business, you do and so do I.'

There is a collection of nearly all Donnan's separates in the Library of University College London, together with a number of interesting letters.

I should like to acknowledge my deep indebtedness to many people, particularly Professor W. E. Garner, F.R.S.

The man and his character

Writing about Donnan involves some of the difficulties encountered in the attempt to describe any famous exponents of anything to those who have seen, heard or known but little of them.

He was tall, good looking, well built and of great physical strength and endurance. Till he smiled his face in repose was often stern and rather sad.

I first met him in Liverpool in 1904 and have a vivid visual impression of the meeting to this day. He appeared in the lecture theatre looking rather set and grim. He gave us a somewhat formidable lecture on thermodynamics which he obviously had at his finger tips. His voice was low and he spoke rapidly. At the end of the lecture he said he would be glad to meet us. The whole Honours Chemistry School was only six strong.

He was then quite a different man and radiated charm and genuine sympathy. We all turned into would-be physical chemists on the spot. For the next two years I was in the closest touch with him, and when the Final Examination was over I did some research under his direction.

Before this his method of teaching us practical physical chemistry was to get us to repeat the experimental work in a recently published paper usually by someone in Germany. If you could not read German you took off your coat and went for it with a grammar and a dictionary; aided by Nernst Schönflies, Allmand and I worked together on one of these exercises.

One could be sure of seeing him at least once a day and listening to his brilliant and illuminating talk.

When the time came for me to work alone he put me on to what proved to be an extraordinarily difficult task. He said that if you mixed together solutions of potassium fluoride, copper sulphate and ferrous sulphate you could obtain metallic copper, cuprous oxide, and a range of other compounds of varying colour. Would I try to work out the systematics of it. I toiled at this for six months and got nothing worth publishing out of it, but it threw me into Donnan's company a great deal and I got to know him really well. It was an invaluable experience for me.

He was a tremendous worker and kept odd hours. To retire at 1 a.m. was early for him; 2, 3 and even 4 a.m. were not infrequent.

He had a great difficulty in starting anything, but when once he did he would work furiously till it was finished. This applied particularly to tasks he had been asked to undertake, like annotating Gibbs. On two occasions at least he worked himself right out and became ill. He was devoted to his friends. Ramsay's lingering illness and eventual death distressed him terribly. He was generous and hospitable to a fault. He had his likes and dislikes, but, like the Scots, as a past president of Magdalen said, 'He was not quarrelsome, but had a fine sense of provocation.'

He had excellent taste and his house in Woburn Square was delightful.

His remarkable old mother and his sisters formed a background for him which anyone who knew him well in his own home will never forget.

The effect he made in a new situation such as his visit to Holland in 1922 was astonishing. Much as he had travelled he did not know Holland and but few of the people there. I went with him as a kind of A.D.C.

He was lecturing on his theory of membrane equilibrium which, thanks to Loeb's book, was very well known at the time. He was always nervous and diffident before an important lecture, but invariably made a success of it when he got under way. The great majority of physical chemists, physicists and physiologists in Holland came to hear him.

At Leyden, for example, there were Einstein, Ehrenfest, Kamerlingh Onnes, Schreinemakers, Crommelin and Jorissen in his audience and a crowd of physiologists.

In the middle of the lecture Ehrenfest, Einstein and Donnan had a triangular discussion each going to the blackboard.

Donnan did more than well, he shone. The lectures at Amsterdam and Utrecht were equally successful. I was present at three meetings of the Deutsche Bunsen Gesellschaft between the wars where Donnan was a marked success. The respect and affection with which he was greeted would have surprised a stranger.

Donnan was without doubt a great international figure and a good European, the present trend towards a United Europe would have delighted him.

His generosity to his colleagues and pupils if less bountiful would have substantially increased the size of his bibliography.

As Sir Charles Goodeve has said his testimonials required a certain amount of discounting but they testified to his goodness of heart.

Like his great friend Collie he engaged the affection and respect of the oncoming generations. At his farewell dinner they acclaimed him as only young men can.

In his obituary notice in *The Times* the writer remarked that he would be remembered as a teacher. He was a good teacher, but his pupils and friends will remember him as an inspiration.

F. A. Freeth

BIBLIOGRAPHY

1892. An attempt to give a simple theoretical explanation of Raoult’s Law of the lowering of vapour-pressure. *Phil. Mag.* (5), **34**, 411.

1895. The pressure of a saturated vapour as an explicit function of the temperature. *Nature, Lond.* **52**, 619.

1896. Versuche über die Beziehung zwischen der elektrolytischen Dissoziation und der Lichtabsorption in Lösungen. Dr Thesis, Leipzig, 1896; *Z. phys. Chem.* **19**, 465.

1896. Dependence of the colour of solutions on the nature of the solvent. *Nature, Lond.* **51**, 55.

1896. Autocatalysis. *Ber. dtsch. chem. Ges.* **29**, 2422.

1897. (With J. H. van ’t Hoff.) Die Maximaltensionen der gesätigten Lösungen von Magnesiumchlorid, Kaliumsulfat, Magnesiumsulfat, Kaliumchlorid und deren Doppelsalzen bein 25°. (Untersuchungen über die Bildungsverhältnisse der ozeanischen Salzablagerungen. Leipzig, 1912, and *S.B. Preuss. Akad. Wiss.* **52**.)

1897. Lord Rayleigh’s proof of van ’t Hoff’s osmotic theorem. *Nature, Lond.* **57**, 43.

1898. The Thomson effect in a binary electrolyte. *Phil. Mag.* (5), **45**, 529.

1898. Theory of the Hall effect in a binary electrolyte. *Phil. Mag.* (5), **46**, 465-472 and *Phys. Soc. Proc.* **16**, 244-251.

1898. The isothermal pressure surface in the case of two single salts and one double salt. *J. Phys. Chem.* **2**, 417.

1899. Ueber die Natur der Seifenemulsionen. *Z. phys. Chem.* **31**, 42-49.

1900. The relative rates of effusion of argon, helium, and some other gases. *Phys. Soc. Proc.* **17**, 112-138; *Phil. Mag.* **49**, 423-446.

1901. Untersuchungen über den Salpeter und den salpetrigen Luftgeist, des Brennen und das Athmen. *John Mayow.* Translated by F. G. Donnan in Ostwald’s *Klassiker der Exakten Wissenschaften*, No. 125, Leipzig.

1901. A theory of collodial solution. *Phil. Mag.* (6), **1**, 647-652; *Z. phys. Chem.* **37**, 735-743.

1902. (With E. C. C. Baly.) The variation with temperature of the surface energies and densities of liquid oxygen, nitrogen, argon and carbon monoxide. *J. Chem. Soc.* **81**, 907-923; *Proc. Chem. Soc.* **18**, 115-116.

1902. (With H. Bassett and C. J. J. Fox.) The colour changes exhibited by the chlorides of cobalt and some other metals, from the stand-point of the theory of electroaffinity. *J. Chem. Soc.* **81**, 939-956; *Proc. Chem. Soc.* **18**, 164.

1902. Condensation of the vapours of organic liquids in presence of dust-free air. *Phil. Mag.* (6), **3**, 305-310.

1903. Theory of capillarity and collodial solutions. *Z. phys. Chem.* **46**, 197-212.

1903. (With R. Le Rossignol.) The velocity and mechanism of the reaction between potassium ferricyanide and potassium iodide in neutral aqueous solutions. *J. Chem. Soc.* **83**, 703-716; *Proc. Chem. Soc.* **19**, 120-121.

1903. Note on the emulsifying action of soap. (Criticism of paper by H. W. Hillyer.) *J. Amer. Chem. Soc.* **25**, 1215-1216.

1903. (With B. C. Burt.) Solubilities and transition points of lithium nitrate and its hydrates. *J. Chem. Soc.* **83**, 335-342; *Proc. Chem. Soc.* **19**, 37-38.

1904. On the reactivity of the alkyl iodides. *Proc. Roy. Soc.* (*Dublin*), **10**, 195-202.

1904. (With Miss K. A. Burke) Chemical dynamics of the alkyl iodides. *J. Chem. Soc.* **85**, 555-589; *Proc. Chem. Soc.* **20**, 46-47.

1905. (With W. D. Donnan.) The surface tension of urine in health and disease. *Brit. Med. J.* p. 1636.

1905. Ueber Komplexbildung, Hydration und Farbe. *Z. phys. Chem.* **53**, 317.

1905. A suggested explanation of the phenomena of opalescence observed in the neighbourhood of critical states. *Rep. Brit. Ass.* **1904**, 504-505.

1906. (With J. T. Barker.) The volume expansion of Portland cement. *J. Soc. Chem. Ind.* **25**, 726.

1908. Kupfer und seine Verbindungen. In Abegg's *Handbuch der anorganischen Chemie.* **2**, Pt. I, p. 469.

1909. (With Miss K. A. Burke.) Chemical dynamics of the alkyl iodides. Pt. II. *Z. phys. Chem.* **59**, 148.

1909. (With W. Schneider.) Colour of aqueous solutions of violuric acid. *J. Chem. Soc.* **95**, 956-959; *Proc. Chem. Soc.* **25**, 148.

1909. (With G. D. Hope.) The calorimetrical analysis of hydrated salts. *Trans. Faraday Soc.* **5**, 244-50.

1909. (With J. T. Barker and B. P. Hill.) Experiments on the current and energy efficiencies of the Finlay electrolytic alkali-chlorine cell. *Electrician*, **63**, 80; *Trans. Faraday Soc.* **5**, 49-67.

1909. The university training of technical chemists. *J. Soc. Chem. Ind.* **28**, 275-280.

1910. The Muspratt Laboratory for Physical and Electrochemistry at the University of Liverpool. *Z. Elektrochem.* **16**, 903-912.

1910. (With H. E. Potts.) Kinetics of the reaction between silver salts and aliphatic iodides. *J. Chem. Soc.* **97**, 1882-1895; *Proc. Chem. Soc.* **26**, 212.

1910. (With H. E. Potts.) The emulsification of hydrocarbon oils by aqueous solutions of salts of fatty acids. *Z. Chem. Ind. Kolloide*, **7**, 208-214.

1910. (With T. W. A. Shaw.) The solubility of oxygen gas in molten silver and its effect in producing blisters. *Brass World*, **6**, 396-397.

1910. (With T. W. A. Shaw.) The solubility of oxygen in molten silver. *J. Soc. Chem. Ind.* **29**, 987-989.

1911. J. H. van 't Hoff. *Nature*, *Lond.* **86**, 84; *Proc. Roy Soc.* A, **86**, xxxix (1912).

1911. (With A. B. Harris.) Osmotic pressure and conductivity of aqueous solutions of Congo red and reversible membrane equilibria. *J. Chem. Soc.* **99**, 1554-1577.

1911. (With A. S. White.) The system palmitic acid, sodium palmitate. *J. Chem. Soc.* **99**, 1668-1679.

1911. (With J. S. Thomas.) The solubility of cuprous oxide in aqueous ammonia solutions and the composition of the cuprous-ammonia complex. *J. Chem. Soc.* **99**, 1788-1796; *Proc. Chem. Soc.* **27**, 213.

1911. The theory of membrane equilibrium in the presence of a non-dialyzable electrolyte. *Z. Elektrochem.* **17**, 572.

1911. (With J. T. Barker.) An experimental investigation of Gibbs' thermodynamical theory of interfacial concentration in the case of an air-water interface. *Proc. Roy. Soc.* A, **85**, 557-574.

1911. (With A. J. Allmand.) Standard electrode with alkaline electrolyte. *Proc. Chem. Soc.* **27**, 70; *J. Chem. Soc.* **99**, 845.

1912. The relations of physical chemistry to modern chemical industry. (Soc. Chem. Ind. Liverpool Meeting, October 1912.)

1914. (With A. J. Allmand.) Ionic equilibria across semipermeable membranes. *J. Chem. Soc.* **105**, 1941-1963; *Proc. Chem. Soc.* **30**, 180-181.

1914. *Chemical research in a modern laboratory.* (Leaflet) London: Constable.

1914. (With G. M. Green.) The variation of electrical potential across a semipermeable membrane. *Proc. Roy. Soc.* A, **90**, 450.

1914. (With H. Rai.) *Bleaching animal or vegetable fats, oils, etc.* British Patent 17,784 (Feb. 5, 1914).

1915. The training of technical chemists. *Chem. Tr. J.* p. 519.

1915. Emulsions and emulsification. *Engineering*, **99**, 551-552.

1916. The graphics of blending in chemical industries. *J. Soc. Chem. Ind.* **35**, 3-7.

1916. The engineer and the chemist. Necessity for co-operation. *Times Engineering Supplement*, p. 113. 28th July.

1916. Chemical science and civilisation. *Nature, Lond.* **97**, 370.
1916. Scientific preparation for the future. *The School World*, **18**, 161.
1916. A Ministry of Science. (Contribution by) *Electrician*, July.
1917. Training of the chemical student for work in the factory. *Chem. Tr. J.* **60**, 206-207; *Chem. News*, **115**, 145.
1917. Mechanism of chemical change. *Engineering*, **103**, 181-182.
1918. The life and work of Sir William Ramsay. (Introduction to *The Life and letters of Joseph Black, M.D.*, by Sir William Ramsay. London: Constable & Co.)
1918. Optical pyrometry in non-ferrous metallurgy. *Trans. Faraday Soc.* **13**, 276-279.
1918. The agglomeration of granular masses. *Trans. Faraday Soc.* **14**, 1.
1918. La science physico-chimique. Decrit-elle d'une facon adequate les Phenomenes Biologiques. *Scientia*, 24 Oct.
1919. (With W. E. Garner.) Equilibria across a copper ferrocyanide and an amyl alcohol membrane. *J. Chem. Soc.* **115**, 1313-1328.
1919. The agglomeration of granular masses. *Trans. Faraday Soc.* **14**, 12-13.
1919. The nitrate industry of Chile. *Chem. Age*, **1**, 634-635.
1919. Heat of reaction and gravitational field. *Nature, Lond.* **104**, 392-393.
1920. (With I. Masson.) Theory of gas scrubbing towers with internal packing. *J. Soc. Chem. Ind.* **39**, 236-41T.
1920 (With W. C.McC. Lewis, E. F. Armstrong and A. S. Shorter.) Colloid chemistry and its industrial applications. (1st report.) (20-105.) 1917. *Rep. Brit. Ass.* (3rd report). (1-154 A.) (2nd report published by M.M.S.O. for D.S.I.R.)
1921-22. Program of investigation for the Chilean nitrate industry. *Caliche*, **3**, No. 9, 387-403; No. 10, 457-470; No. 11, 483-505.
1922. Auxiliary international languages. (Evening Lecture, Roy. Instn. March 1922.) *Nature, Lond.* **109**, 491.
1923. Some aspects of the physical chemistry of interfaces. *Chem. & Ind.* **42**, 892-901; *Chem. Tr. J.* **73**, 332-334; *Chem. News*, **127**, 209-212; *Chem. Age*, **9**, 282-284.
1923. Some control formulas in leaching and evaporation. With special reference to the Chilean nitrate industry. *Proc. Chem. Eng. Group. Soc. Chem. Ind.* **42**, 440-2T. 5-6 A. 15.
1924. The theory of membrane equilibria. *Chem. Rev.* **1**, 73, 90.
1925. The influence of J. Willard Gibbs on the science of physical chemistry. Centenary Celebrations, Sept. 1924. *J. Franklin Inst.* **199**, 457-484.
1925. Some aspects of adsorption. *J. Soc. Dyers & Colour.* p. 206.
1926. The phase rule of Gibbs in industry. *Chem. Weekblad*, **23**, 422.
1927. Concerning the applicability of thermodynamics to the phenomena of life. *J. Gen. Physiol.* (Loeb Mem. Vol.) **8**, 685.
1927. Heike Kamerlingh Onnes. *Proc. Roy. Soc.* A, **113**, i.
1927. The Chemical Department at University College London. *J. Chem. Educ.* **4**, 819.
1928. Thermodynamic theory of some equilibria in contact with semipermeable membranes. *Inst. intern. Chimie. Solvay.* 3me conseil. pp. 241-255.
1929. Résumé on investigations on membrane equilibria and membrane potentials. *Nat. Research Council Bull.* No. 69, 51-55.
1929. Physical chemistry in the service of biology. (First Liverside Lecture.) *J. Chem. Soc.* pp. 1387-1398.
1929. (With K. Krishnamurti.) The scattering of light in sols and gels. (Colloid Symposium Annual Meeting. 1929.)
1929. The phenomena of life. *Scientia*, p. 317.
1930. Theory of ion distribution equilibria in a gel system with variable micelle distribution. *Kolloid-Z.* **51**, 24-27.
1931. Transformation of radiation into matter. *Z. phys. Chem.* (Bodenstein-Festband) pp. 131-14.

1931. Matter and radiation (Lecture to Metropolitan-Vickers Elec. Co. Trafford Park). *Nature, Lond.* **128**, 290-292.
1931. Electrochemistry. *Times Faraday Cent. Supplement*, p. 18. Sept.
1931. Rt Hon. Lord Melchett, D.Sc., LL.D., F.R.S. (Obituary Notice). *J. Chem. Soc.* p. 3372.
1932. Chemistry and radiation in *Science today and tomorrow*, London: Williams and Norgate.
1932. Membrane equilibria. *Kolloid-Z.* **61**, 160-167.
1922. (With E. A. GUGGENHEIM.) Exact thermodynamics of membrane equilibrium. *Z. phys. Chem.* A, **162**, 346-360.
1933. The Ostwald Memorial Lecture. (Delivered 27.1.1933.) *J. Chem. Soc.* pp. 316-332.
1933. Some considerations relating to membrane equilibria and the secondary swelling of protein gels. *J. Int. Soc. Leather Trades Chemists*, p. 136.
1933. Note on the thermodynamic functions of radiation. (Sir P. C. Ray 70th Birthday Comm. Vol.) *J. Indian Chem. Soc.* p. 61.
1934. The exact thermodynamics of membrane equilibrium. *Z. Phys. Chem.* A, **168**, 369-380.
1935. Molar (micellar) mass, electrovalency of ions and osmotic pressure of colloidal electrolytes. *Trans. Faraday Soc.* **31**, 80-98.
1936. The chemist in industry: introductory article. *J. Careers*, **15**, 73.
1936. Integral analysis and the phenomena of life. 1. *Acta Biotheoretica*, **2**, 1.
1937. (With E. TELLER and B. TOPLEY.) Fluctuations of composition in a system of molecules in chemical equilibrium. *Phil. Mag.* **24**, 981-1001.
1937. Integral analysis and the phenomena of life. 11. *Acta Biotheoretica*, **3**, 43.
1938. Quatercentenary of the University of Coimbra. *Nature, Lond.* **141**, 63.
1938. Retrospect and prospect. Presidential Address. *J. Chem. Soc.* p. 575.
1939. Ludwig Mond Memorial Lecture. (Delivered Roy. Instn. 15 March 1939; Delivered Inst. of Chem. 12 May 1939.)
1939. The role of osmotic pressure in the development of chemical and biochemical science. Presidential Address. *J. Chem. Soc.* pp. 707-714.
1939. The specific laws of theoretical biology. *Z. Angew. Chem.* **42**, 469-471.
1939. Sir Robert Ludwig Mond. *Proc. Roy. Soc. Edinb.* **59**, 273.
1939. Baron Joji Sakurai. *Nature, Lond.* **144**, 234.
1941-42. The linkage of physico-chemical processes in biological systems. *Nature, Lond.* **148**, 723-724; **149**, 383.
1942. Herbert Freundlich. 1880-1941. *J. Chem. Soc.* pp. 646-654; *Obit. Not. Roy. Soc.* **4**, 27.
1947. The Ernst Julius Cohen Memorial Lecture (Bibliography & Obituary). *J. Chem. Soc.* pp. 1700-1706.
1948. Edward Charles Cyril Baly, 1871-1948. *Obit. Not. Roy. Soc.* **6**, 7; *J. Chem. Soc.* p. 1721
1950. (With R. C. ROSE.) Osmotic pressure, molecular weight, and viscosity of sodium alginate. *Canad. J. Res.* **28**B, 105-113.
1952. Recollections of van 't Hoff. *Chem. Weekblad.* **48**, 643-644.
1953. The first fifty years of the Faraday Society, 1903-1953. Some personal reminiscences. *Trans. Faraday Soc.* **49**, 511-514.
1953. (With J. SWINBURNE.) Addresses by Founder Members of the Faraday Society. *Trans. Faraday Soc.* **49**, 471-472.

Editorial work

Text books of physical chemistry (Longmans Green & Co.)

Reports on colloid chemistry and its general and industrial applications. (Six reports published by a Committee of the British Association for the Advancement of Science, Chairman F. G. Donnan.)

Commentary on the scientific writings of J. Willard Gibbs. Vol. 1. Thermodynamics. Yale University Press.

J. Evershed

JOHN EVERSHED

1864-1956

JOHN EVERSHED was born on 26 February 1864 at Gomshall in Surrey. He was the seventh child of John Evershed and Sophia, daughter of David Brent Price of Portsmouth. John Aubrey, F.R.S., in his *History of Surrey*, refers to the Evershеds of Evershed, in the parish of Ockley 'a family here rather before than since the Conquest'. The earliest member of the family, of whom there is record, was Thomas de Evereshavede, a witness to a charter in 1240. The Evershеds were yeoman farmers and have been traced from father to son living on the same farm for 500 years, until 1716, when the last John Evershed of Evershed, Sheriff of Surrey, died without heirs. Evershed's grandfather, John Evershed of Albury, was descended from a younger branch of the family, who had continued as yeoman farmers in the neighbouring parish of Newdigate. He was the last of the branch to carry on an unbroken family tradition as yeoman farmers in Surrey for over 600 years.

Evershed's maternal great-grandfather kept a shop in Portsmouth where he sold nautical almanacs and charts; he was well known to Nelson. The only relative of Evershed's of scientific attainments was his brother Sydney, who invented electrical apparatus and electrical measuring instruments for the Navy and carried out researches in permanent magnetism.

Evershed was educated at a preparatory school of the Unitarian persuasion at Brighton, where he had Neville Chamberlain as a school-fellow, and at a more advanced school at Croydon, where, as he said 'by a lucky accident I passed the only examination I have ever been subjected to'. He remembered stealing out of bed at Brighton in the middle of the night to see a fine comet (Coggia 1874). While still at school, when the planet Mars was in a near opposition (1877), he constructed a telescope with odd lenses. He had to observe Mars through his telescope and a plate-glass window as well, as in those days night air was supposed to be harmful to young people. Later with a spectroscope made out of two lenses from an old disused pair of opera glasses and a small but perfect prism given to him by his brother he was thrilled on seeing the sodium line D of the solar spectrum split into two fine lines and this experience probably gave the final direction to his scientific activities. The early training in workshop practice that he had obtained from his brother served him well, not only in these early attempts to design and build up instruments himself, but also throughout his whole life, especially in India.

To his brother Evershed also owed his early introduction into active scientific circles. For he brought Raphael Meldola, F.R.S., into the family circle and through Meldola Evershed was able from time to time to attend Friday evening lectures at the Royal Institution and on one occasion to visit the Royal Society, where he was introduced to Charles Darwin. Evershed even contributed an observation to a book on the evolution of caterpillars by Weissmann which Meldola was translating. Later this side of Evershed's interests took the shape of a very fine collection of Lepidoptera, both Indian and British. He became extremely proficient at photographing butterflies in colour. While employed by a firm of manufacturers in London and other centres of industry on analyzing oils and other products he was also experimenting on the radiation of heated gases. He demonstrated that the coloured vapours of iodine, chlorine and bromine emitted continuous spectra in the visible region at the moderate temperature of a red-hot solid, but gave a discontinuous absorption spectrum by transmitted light. He also proved that heat alone to the exclusion of chemical or electrical stimulation was sufficient to produce the discontinuous *D* radiation from vaporized sodium metal, and that the change from continuous to line radiation was largely a matter of decreasing density.

But his real interest lay in solar research. In 1890 he was a founder member of the British Astronomical Association and he was Director of the Section of Solar Spectroscopy 1893-9 and of the Section of Spectroscopy 1924-6. Fired by Lockyer's method of observing solar prominences he built himself a spectroscope with a battery of prisms and in 1890 he commenced a long series of observations of prominences from the private observatory that he had established at Kenley. Between 1890 and 1905 he recorded the positions of 13458 prominences and noted the changes in heliographic latitude during a solar cycle. In 1892 he succeeded in photographing prominences in the hydrogen line $H\beta$. This was independent of Hale's invention of the spectroheliograph in the same year. By February 1893 Evershed was also getting solar images showing the brilliant flocculi around sunspots, having devised a method of monochromatic photography of the whole disk of the sun. Correspondence in *Knowledge* had brought Evershed into touch with A. Cowper Ranyard who introduced him to Hale. Ranyard, who died in 1894, presented Evershed with a fine 18-inch reflecting telescope and a small spectroheliograph which Hale had designed and used in an unsuccessful attempt to photograph the corona in full daylight from Mount Etna. Evershed designed a new form of spectroheliograph employing a large direct-vision prism for use with his reflecting telescope.

One of the directors of the company for whom he was working, W. H. Stead, was interested in scientific work and Evershed was granted leave to join eclipse expeditions. He went with the British Astronomical Association expedition to Norway for the total eclipse of 9 August 1896, hoping to get the emission spectrum of the reversing layer for comparison with the Fraunhofer spectrum. His station on the shore of the Varanger Fjord was, however, clouded out. It was at this eclipse that he met Miss Mary Acworth Orr, who later, as Mrs Evershed, became an active co-worker with him in his solar research work. In 1898 he joined another British Astronomical Association expedition to Talni in India for the total eclipse of 22 January 1898. There with a home-made prismatic camera he secured beautiful spectra extending far into the ultra-violet; he was the first to photograph the emission continuum at the head of the Balmer series. A detailed study of the flash spectrum showed that it represented the upper portion of a stratum of gas which by its absorption gives the Fraunhofer spectrum; Evershed also discussed the abnormal strength of the enhanced or spark lines of iron. In 1900 he went to a station near Maelma in Algeria close to the southern limit of the zone of totality, so chosen as to allow a series of 'flash spectra' to be secured over a longer range of time and of heliographic latitude. Unfortunately the diameter of the moon adopted by the Nautical Almanac Office in calculating the data of the eclipse was too large and instead of having a total eclipse of 30 seconds, as Evershed had intended, he found himself just outside the belt of totality. He did, however, secure a number of spectra, including one covering the south polar region, justifying his idea of choosing a site on the edge of the belt. The detailed measurements of the spectra obtained at the two eclipses of 1898 and 1900 were communicated to the Royal Society.

Further eclipses observed by Evershed were on 30 August 1905 at Pineda de la Sierra in Spain, on 21 September 1922 at Wallal in West Australia and on 29 June 1927 in Yorkshire. Evershed was unlucky on all three occasions. At his last eclipse he had a prismatic camera of 21 ft. focal length with two 45° Hilger prisms of 6-inch effective aperture which, he stated, were the finest prisms of that size that had ever been made. At all the eclipses his instruments, except the optical parts, were home-made. One of his early papers that should be mentioned here is on the cause of the Gegenschein. He took the view which is now becoming popular that the Gegenschein is really due to a gaseous tail of the earth pointing away from the sun.

In the year 1906 Evershed, on the initiative of Gilbert Walker (later Sir Gilbert) and with the backing of Sir William Huggins, P.R.S., was offered by the India Office the post of Assistant Director of the Kodaikanal Observatory in Southern India. He gladly accepted and travelled out to India with Mrs Evershed *via* the U.S.A. and Japan. In America he met the Pickerings, Barnard, Frost, St John and Campbell and he spent a month at Mt Wilson with Hale. On arrival at Kodaikanal he commenced a study of the spectrum of sunspots, his first paper giving a list of lines in the ultra-violet, which differed in intensity in the spectra of a sunspot and of the adjacent photosphere. Comet Daniel (1907*d*) also claimed his attention and he used his eclipse prisms to make a prismatic camera attached to a 6-inch equatorial. He identified cyanogen bands in both the nucleus and the tail of the comet. Three years later he secured spectra of Halley's comet: he identified cyanogen and the Swan spectrum in the head, with a continuous spectrum showing the stronger Fraunhofer lines: the tail showed bands of carbon monoxide. Attempts were made to secure photographs of the comet's nucleus in transit across the sun's disk, but poor weather conditions and the diffuseness of the comet's head prevented success. The tail was traced on clear mornings up to the Milky Way, more than 100° from the head.

Evershed's chief work in his first years at Kodaikanal was on the spectrum of sunspots and it was summed up in a *Memoir* which he published in 1909. The chief points that he noted were the following:

(1) The sunspot spectrum is constant in character and consists mainly of intensified Fraunhofer lines.

(2) The high temperature or spark lines are weakened.

(3) The general absorption is strictly continuous and similar to that at the limb.

(4) The pressure is slightly less than in the reversing layer over the surrounding photosphere.

(5) The movement of the umbral gases (excluding hydrogen and calcium) perpendicular to the sun's surface is small.

(6) The umbral and penumbral gases are in motion radially outwards from the spot centre and parallel to the sun's surface. The motion accelerates outwardly, velocities of 2 km/s having been measured at the outer limits of the penumbra where the motion is usually lost in a very short distance, though it occasionally extends a considerable distance beyond the penumbra. Gases of the higher chromosphere (hydrogen and calcium) show a tendency to move inwards with a diminishing velocity toward the spot centre.

The last item (6) is known as the Evershed effect: it was first observed by him in a large sunspot on 5 January 1909, on an early morning with good definition and unusual transparency following a week of incessant heavy rains. His success in observing and measuring these radial motions, which had hitherto escaped observation, was due in part to the careful methods he had adopted in measuring small shifts in sunspot spectra and in part to the good use that he made of a large spectrograph that he built for a 6-inch grating by Michelson that he had received from America. One further feature that he observed in the lower chromosphere was a slight rotational velocity of the gases above the penumbra: the mean rotational velocity was 0·35 km/s and the direction of rotation was opposite to that suggested by the spiral stream-lines in Hale's Hα spectrograms. Evershed's apparent rotational velocity changed sign in passing from the northern to the southern hemisphere. It was, however, not a constant and regular feature as was the radial movement.

Evershed's skill in the designing and building of instruments was made use of in his early years at Kodaikanal to improve and get into working order the Cambridge spectroheliograph; some years later, to enable the Hα line to be employed as well as the *K* line an autocollimating spectrograph was bolted on to the framework of the existing instrument; a large Michelson grating was used or, alternatively, an autocollimating train of prisms; simple arrangements enabled the solar images in *K* and Hα light to be obtained with very little delay between exposures. With the photographic emulsions available in 1914 exposures needed on Hα were too long for prominences but good spectroheliograms of the disk in Hα light were obtained.

Evershed continued at Kodaikanal the systematic study of solar prominences that he had commenced at Kenley in 1890. A series of papers on the distribution of prominences and on individual prominences of unusual interest grew into a valuable memoir which he and Mrs Evershed published in 1917 in the *Memoirs of the Observatory*. In the course of an eleven year solar cycle, 1904-1914 inclusive, nearly 60000 prominences had been observed at Kodaikanal, including all down to the smallest visible, and there was enough material for the Eversheds to examine the distribution in latitude of prominences and to establish the movements of the zones of prominence activity, during the solar cycle. The authors were able to confirm Evershed's earlier suggestions of the existence of two zones of which one moved out to the poles at sunspot maximum. They found a definite excess of prominence numbers on the East limb of the sun compared with the West limb and an eastern excess also for prominences photographed on the disk; they failed to find any physical explanation, being loath to believe that the earth could influence the sun in this matter, when no similar evidence could be detected for the other planets. A link between metallic prominences and sunspots was established. Measurements of the *H* and *K* lines of calcium showed that the rate of rotation at a level of about 45″ above the limb was higher than that of the chromosphere. Many beautiful pictures revealed the motions and changing forms of prominences, subjects which have more recently been studied in closer detail through the spectroheliokinematograph of McMath and by direct cinematography with the Lyot coronagraph and the Lyot filter. Links between the dark hydrogen filaments on the sun's disk and bright prominences on the limb were examined.

Two main classes of prominences were distinguished, the large high prominences not limited to the sunspot zones and the jets, rockets and arches associated with sunspots. Evershed's pioneer work in the field of prominence studies raised a number of questions some of which are still unresolved.

On the retirement of Michie Smith in 1911 Evershed succeeded him as Director of the Kodaikanal Observatory. Royds joined him as Assistant Director and interest and activity turned more and more in the direction of the exact measurement of wave-lengths of spectrum lines. Much lower pressures in the reversing layer than had been generally assumed were indicated by the behaviour of the lines in the solar spectrum—the small shift to the red of spectrum lines at the centre of the disk. Limb spectra were measured and compared with the spectrum at the centre of the disk for a study of solar rotation and of the shift towards the red of lines at the limb. At one time Evershed was inclined to attribute this shift to motion, another indication of a puzzling earth effect indicated by Mrs Maunder's work on sunspots and his own work on prominences. Later when the Einstein gravitational displacement was recognized as a factor to be taken into account Evershed made further studies of the question. His final view was that the Einstein effect accounted for most of the red shift at the limb but that there remained a definite unexplained residual shift. The problem was left unsolved.

Several other papers by Evershed published before he left Kodaikanal call for mention. High dispersion spectrograms were secured on Venus, showing that the line shift was unaltered when light reflected from the far side of the sun was examined: again spectra were obtained of *Nova Aquilae* 1918 during the first two weeks after the outburst of radiation. Evershed deduced from the initial high velocity outward of the gases, compared with the velocities in prominences with which he was familiar, that only the gases of the star's original chromosphere, lying outside the intensely luminous photosphere would be driven out if the repulsive force was light pressure. He emphasized the presence of sharp non-displaced absorption of *H* and *K* lines in the spectrum and identified their source with interstellar gas clouds, a view now regularly accepted but not so generally held in 1918. A high dispersion spectrogram was secured of *Sirius* and Evershed pointed out the width of the absorption lines, especially of the hydrogen lines, compared with the solar spectrum photographed, with the same instrumental set up, in light reflected from Venus.

One experience of Evershed's while still in India must be mentioned—a visit to Kashmir in 1913 on three months' leave. He found extremely good observing conditions for solar work in the Valley of Kashmir—in a level plain, containing a river and much wet cultivation of rice, 8000 ft. above sea level and completely surrounded by high mountains. The solar definition he found to be extremely good at all times of the day, and unlike most high level stations to be best near noon and in hot weather. He established a temporary observatory near Srinagar in 1915-16 obtaining photographs of prominences and sunspots of the highest quality. Between his two visits to Kashmir Evershed was lent by the Government of India to the New Zealand Government to advise them on a proposal for establishing an observatory in the South Island. His experience, based on observations in various localities, was that the best seeing was to be obtained in low-level plains near the sea and not on high mountain sites nor even hilltops. His views were contrary to the ideas of the prospective donor of the observatory and the funds available were ultimately devoted to another purpose.

In 1923 Evershed retired from the Directorship of the Kodaikanal Observatory and returned to England. On his final trip down the ghaut road from the observatory to his ship a tiger bounded across the road in front of his car—the only time that he saw a tiger during his sixteen years in India. On his return to England he established a private observatory at Ewhurst where he continued solar observations for the best part of a further thirty years. He retained his interest in exact wave-lengths in the solar spectrum, sunspots and prominences. Working with large hollow prisms filled with ethyl cinimate and giving very high resolving power, he was able to discover that lines which are Zeeman triplets in the umbra of a spot are doublets in the penumbra. The two components are oppositely polarized. The separation of the doublet in the penumbra increases up to the edge of the umbra where the central component appears; the separation of the triplet increases up to the centre of the umbra. The magnetic field then is greatest when the outward radial velocity is zero and weakens as the radial velocity increases. No explanation has yet been given of the apparent change of direction of the magnetic field at the boundary of the umbra and penumbra.

Evershed's refined studies of solar spectrum wave-lengths led Hale to consult him when difficulties arose in the determination of the sun's general magnetic field. Evershed, using his positive on negative method for detecting small displacements of lines (a method that he had developed and used successfully in Kodaikanal), measured a number of Mt Wilson plates and came to the definite conclusion that the small shifts on the plates that he measured were not due to a general magnetic field nor to local magnetic fields but were Doppler effects due to motion. This conclusion was in part based on the behaviour of an iron line which gave a great Zeeman separation in the sunspot spectrum but gave no larger shifts than the other lines measured on the Mt Wilson plates used for the detection of the sun's general magnetic field.

Other work carried out during Evershed's later years at Ewhurst included further studies of the solar rotation from the *H* and *K* lines in prominences: he found that the mean daily angular rotation at 30″ above the photosphere was between 1° and 2° greater than in the reversing layer and increased with the increase of solar activity; the shift towards the red of the *H* and *K* lines in prominences showed an excess over the relativity effect approximately the same as that given by the iron lines of the reversing layer at the limb. He found similar excess shifts at the limb in the reversing layer for the lines of calcium, aluminium and sodium. The iron lines of medium intensity give shifts at the centre of the sun in close agreement with the relativity effect, but the detection of small Doppler shifts in the spectrum lines representing large areas of the solar surface shows the difficulty of disentangling the Einstein effect with any certainty from the displacements resulting from convection currents and other movements over the sun's surface.

Evershed continued his active observations till 1953 when he finally closed down his observatory at the age of 89. He then presented some of his instruments to the Royal Greenwich Observatory at Herstmonceux Castle.

Referring to the steadily increasing accuracy of his measures he made the following remark:

> 'I began with a prism containing just a cubic centimetre of glass, I end with a prism containing two litres of liquid; instead of barely splitting the *D* lines, D_1 and D_2 appear now half an inch apart, with a large number of

solar lines between.' His keenness to make observations whenever conditions allowed is shown by the following statement from his doctor: 'He was an impossible patient and however ill would always refuse to leave his underground observatory. I well remember having to examine his chest as best I could while he was engaged in taking photos of the sun. At the age of 75 I found that he had auricular fibrillation and warned him that he must go reasonably quietly for a period of time. On my next visit, as if to defy his pathology, I found him felling trees with an axe.'

Evershed was elected a Fellow of the Royal Society in 1915 and was awarded the Gold Medal of the Royal Astronomical Society in 1918 for his investigation of radial motion in sun-spots and other contributions to astrophysics. He was made Companion of the Order of the Indian Empire in 1923 on retirement from Kodaikanal. He married Miss Mary Acworth Orr in 1906. She died in 1949. In 1950 he married Miss Margaret Randall, who survives him. There were no children. He died on 17 November 1956, aged 92.

In conclusion a quotation from Newall's address in presenting the Gold Medal of the Royal Astronomical Society to Evershed may not be amiss: 'There is much in our medallist's career which is a reminder of the scientific life of Sir William Huggins. They come from the same English neighbourhood and began as amateurs of the best kind. They both possess the same kind of scientific aptitude'.

F. J. M. Stratton

BIBLIOGRAPHY

1893. Explosions in the sun. *Knowledge*, **16**, 207, 232.
1895. Experiments on the radiation of heated gases. *Phil. Mag.* (5), **39**, 460.
1897. The cause of the darkness of sunspots. *Astrophys. J.* **5**, 244.
1898. Sunspot radiation. *Observatory*, **21**, 404.
1898. Note on some results obtained with a small prismatic camera at the Eclipse Camp at Talni. *Mon. Not. R. Astr. Soc.* **58**, 298.
1899. The cause of the Gegenschein. *Observatory*, **22**, 57.
1899. The parallax of the Gegenschein. *Observatory*, **22**, 272.
1900. Solar eclipse of 28 May 1900. Preliminary report of the expedition to the south limit of totality to obtain photographs of the flash spectrum in high solar latitudes. *Proc. Roy. Soc.* A, **67**, 370.
1901. Wave-length determinations and general results obtained from a detailed examination of spectra photographed at the solar eclipse of 22 January 1898. *Phil. Trans.* A, **197**, 381.
1901. Abstract of the above paper. *Observatory*, **24**, 82; *Astrophys. J.* **13**, 223.
1902. The flash spectrum. *Observatory*, **25**, 198, 272.

1903. Solar eclipse of 28 May 1900—General discussion of spectroscopic results. *Phil. Trans.* A, **201**, 457.

1904. Sunspots and magnetic storms. *Observatory*, **27**, 129.

1904. The Rumford spectroheliograph of the Yerkes Observatory. *Observatory*, **27**, 164.

1906. Report of the expedition to Pineda de la Sierra, Spain, for the eclipse of 30 August, 1905. *Proc. Roy. Soc.* A, **77**, 42.

1907. Distribution of prominences in latitude during the year 1906 on 156 days in the first half of the year, and 105 days in the second half. *Mon. Not. R. Astr. Soc.* **67**, 477.

1907. The ultra-violet region in sunspot spectra. *Mon. Not. R. Astr. Soc.* **68**, 12.

1907. The spectrum of comet 1907 d (Daniel). *Mon. Not. R. Astr. Soc.* **68**, 16.

1908. Solar prominences in 1907, observed at the Kodaikanal Observatory. *Mon. Not. R. Astr. Soc.* **68**, 515.

1908. Helium absorption in the sun. *Observatory*, **31**, 212.

1908. Sunspots and solar temperature. *Observatory*, **31**, 462.

1908. A large prominence. *Astrophys. J.* **28**, 79.

1908. Note on the wave-length of Hδ and Hϵ in the solar spectrum. *Astrophys. J.* **28**, 162.

1909. Water-vapour in the spot spectrum. *Observatory*, **32**, 101.

1909. Radial motion in sunspots. *Kodaikanal Observatory Bull.* No. 15; *Mon. Not. R. Astr. Soc.* **69**, 454; *Observatory*, **32**, 291.

1909. Sunspots and solar temperature. *Observatory*, **32**, 135.

1909. Pressure in the reversing layer. *Kodaikanal Observatory Bull.* No. 18; *Observatory*, **32**, 254, 362.

1909. Spectrum of sunspots. *Mem. Kodaikanal Observatory*, **1**, Part 1.

1910. Radial motion in sunspots, second paper. *Mon. Not. R. Astr. Soc.* **70**, 217.

1910. (With Michie Smith.) Observations of Halley's Comet and its spectrum; the transit across the sun's disc. Observations of the tail before and after the day of transit. *Kodaikanal Observatory Bull.* No. 20; *Mon. Not. R. Astr. Soc.* **70**, 605, 610, 612.

1910. Note on the magnetic field in the sunspot of September 1909. *Kodaikanal Observatory Bull.* No. 22.

1911. On the angular speed of rotation of a long-enduring prominence. *Astrophys. J.* **33**, 1.

1911. The auto-collimating spectroheliograph of the Kodaikanal Observatory. *Mon. Not. R. Astr. Soc.* **71**, 719.

1911. (With M. A. Evershed.) Dante and mediaeval astronomy. *Observatory*, **34**, 440.

1912. On the presence of radium and elements of the inactive group in the chromosphere. *Kodaikanal Observatory Bull.* No. 27.

1912. Radium and the chromosphere. *Observatory*, **35**, 360.

1912. On the relative number of prominences observed on the Eastern and Western Limbs. *Kodaikanal Observatory Bull.* No. 28.

1913. (With M. A. Evershed.) Some types of prominences associated with sunspots. *Mon. Not. R. Astr. Soc.* **73**, 422.

1913. (With T. Royds.) Spectroscopic measures of the solar rotation made at the Kodaikanal Observatory. *Mon. Not. R. Astr. Soc.* **73**, 554.

1913. A new method of measuring small displacements of spectrum lines. *Kodaikanal Observatory Bull.* No. 32.

1913. A new interpretation of the general displacement of the lines of the solar spectrum towards the red. *Kodaikanal Observatory Bull.* No. 36.

1914. Displacement of the solar lines towards the red. *Observatory*, **37**, 124.

1914. (With T. Royds.) On the displacement of the spectrum lines at the sun's limb. *Kodaikanal Observatory Bull.* No. 39.

1914. Note on the radial movement in sunspots. *Astrophys. J.* **40**, 156.

1914. Report on the conditions for astronomical work in Kashmir. *Kodaikanal Observatory Bull.* No. 42.

1914. The general shift of the Fraunhofer Lines towards red. *Observatory*, **37**, 388.

1915. (With Narayana Ayyar.) The displacements of the enhanced lines of iron at the centre of the sun's disc. *Kodaikanal Observatory Bull.* No. 46.

1916. Anomalous dispersion in the sun. *Observatory*, **39**, 59.

1916. (With T. Royds.) On the change of wave-length of the iron lines in passing from the centre of the sun's disk to the limb. *Kodaikanal Observatory Bull.* No. 49.

1916. New measures of radial motion in sunspots. *Kodaikanal Observatory Bull.* No. 51.

1916. Large prominences. *Observatory*, **39**, 392.

1916. Anomalous dispersion in the sun. *Observatory*, **39**, 432.

1917. The solar prominence of 26 May 1916. *Kodaikanal Observatory Bull.* No. 55.

1917. (With M. A. Evershed.) Results of prominence observations. *Mem. Kodaikanal Observatory*, **1**, Part 2.

1917. Einstein effect and eclipse of 29 May 1919. *Observatory*, **40**, 269.

1918. Displacement of the cyanogen band in the solar spectrum. *Observatory*, **41**, 371.

1918. Positive on negative method of measuring spectra. *Observatory*, **41**, 443.

1919. Displacement of the solar lines reflected by Venus. *Observatory*, **42**, 51.

1919. Calcium clouds in the Milky Way. *Observatory*, **42**, 85.

1919. Pulsation theory of Cepheids. *Observatory*, **42**, 124.

1919. The spectrum of Nova Aquilae. *Mon. Not. R. Astr. Soc.* **79**, 468.

1919. The moon in daylight. *Observatory*, **42**, 339.

1919. Is Venus cloud covered? *Mon. Not. R. Astr. Soc.* **80**, 7.

1920. The solar prominence of 29 May, 1919. *Mon. Not. Astr. Soc.* **80**, 8.

1920. Displacement of the solar lines and Einstein's prediction. *Observatory*, **43**, 153.

1920. On the displacements of the triplet bands near λ3883 in the solar spectrum. *Kodaikanal Observatory Bull.* No. 64.

1921. (With Chidambara Ayyar.) An apparent influence of the earth on solar prominences. *Kodaikanal Observatory Bull.* No. 67.

1921. The relativity shift in the solar spectrum. *Observatory*, **44**, 243.

1922. Widened lines in the spectrum of Sirius. *Mon. Not. R. Astr. Soc.* **82**, 392.

1922. The spectrum of Sirius. *Observatory*, **45**, 296.

1923. Note on the corona of 1908. *Mon. Not. R. Astr. Soc.* **83**, 153.

1923. Report of the Indian Eclipse Expedition to Wallal, West Australia. *Kodaikanal Observatory Bull.* No. 72.

1923. The Einstein effect in the solar spectrum. *Observatory*, **46**, 299.

1924. Stationary clouds in space. *Observatory*, **47**, 53.

1925. On some measures of the solar rotation at different levels in the chromosphere. *Mon. Not. R. Astr. Soc.* **85**, 607.

1926. The green flash. *Observatory*, **49**, 369.

1927. On an emission line in the solar spectrum. *Mon. Not. R. Astr. Soc.* **87**, 350.

1927. The solar rotation and the Einstein displacement from measures of the *H* and *K* lines in prominences. *Mon. Not. R. Astr. Soc.* **88**, 126.

1928. High dispersion spectra. *Mon. Not. R. Astr. Soc.* **89**, 175.

1929. The solar rotation derived from the *H* and *K* lines in prominences (second paper). *Mon. Not. R. Astr. Soc.* **89**, 259.

1929. Emission lines in the absorption bands of *H* and *K*. *Mon. Not. R. Astr. Soc.* **89**, 566.

1929. The normal wave-lengths of the calcium lines *H* and *K* and the relativity shift of these lines in the prominences and chromosphere. *Mon. Not. R. Astr. Soc.* **90**, 186.

1930. The wave-length of $H\epsilon$ and displacements of the hydrogen lines in the sun. *Mon. Not. R. Astr. Soc.* **90**, 752.

1931. The shift towards red of the calcium, aluminium and iron lines in the solar spectrum. *Mon. Not. R. Astr. Soc.* **91**, 260.

1931. Note on an apparent increase in the equatorial rotational velocity of the sun. *Mon. Not. R. Astr. Soc.* **92**, 105.

1932. (With M. A. Evershed.) Recent work at Arcetri. *Observatory*, **55**, 254.

1933. A new method of using a spectrograph for solar rotational work. *Mon. Not. R. Astr. Soc.* **93**, 165.
1933. On the detection of small Doppler shifts in the spectrum of the reversing layer. *Mon. Not. R. Astr. Soc.* **94**, 96.
1933. The central intensities of the Fraunhofer lines. *Observatory*, **56**, 275.
1935. The solar rotation and shift towards red of the *H* and *K* lines in prominences (third paper). *Mon. Not. R. Astr. Soc.* **95**, 503.
1936. The red shift of the iron lines at the edge of the sun. *Mon. Not. R. Astr. Soc.* **96**, 152.
1936. Notes on a liquid prism spectrograph. *Observatory*, **59**, 309.
1937. The problem of the red shift in the solar spectrum. *Observatory*, **60**, 266.
1938. The red shift of the *D* lines of sodium in the sun. *Mon. Not. R. Astr. Soc.* **98**, 195.
1938. George Ellery Hale. *Observatory*, **61**, 163.
1938. The problem of the red shift in the solar spectrum (Reply to S. M. Sulaiman). *Observatory*, **61**, 299.
1938. New measures of the wave-lengths of the calcium lines *H* and *K*. *Mon. Not. R. Astr. Soc.* **99**, 120.
1939. Note on the Zeeman effect in sunspot spectra. *Mon. Not. R. Astr. Soc.* **99**, 217.
1939. Measures of the relative shift of the line 5250.218 and neighbouring lines in Mount Wilson solar magnetic field spectra. *Mon. Not. R. Astr. Soc.* **99**, 438.
1940. Sunspots and magnetic storms. *Observatory*, **63**, 47.
1944. Magnetic effect in sunspot spectra. *Observatory*, **65**, 190.
1945. New measures of the sodium line D_1 in the solar spectrum. *Mon. Not. R. Astr. Soc.* **105**, 200.
1945. The solar rotation and shift towards red measured in prominence spectra. *Mon. Not. R. Astr. Soc.* **105**, 204.
1955. Recollections of seventy years of scientific work. *Vistas in Astronomy*, **1**, 33.

A. Felix

ARTHUR FELIX

1887-1956

The death of Arthur Felix has deprived medical bacteriology of one of its unique personalities. Trained as a chemist, chance circumstances brought him into association with Edmund Weil, the eminent German bacteriologist, during the first world war and thus launched him on a career which was, in its entirety, the logical development of the important discoveries with which the names of these two men are linked. The first of these was the diagnostic test in typhus fever known as the Weil–Felix reaction and investigation of this led, in turn, to the discovery of the H and O antigens of organisms of the *Proteus* and enteric groups. Following Weil's untimely death in 1922, Felix brilliantly applied their observations on these antigens to the solution of an apparently insuperable difficulty that had arisen in the serodiagnosis of enteric fevers in vaccinated persons. The inevitable result of this work was that he became interested in problems of effective serum treatment and vaccine prophylaxis and in 1934 he demonstrated that virulent strains of the typhoid bacillus possess an additional antigen which he named the Vi antigen. One of the sequels to this discovery was the development of the epidemiologically important method of typhoid Vi phage typing. Felix adopted this in 1940 and when the war ended there began the final phase of his career which was increasingly devoted to the development and international standardization of enteric phage typing.

Little is known with certainty about Felix's early life; he was not given to reminiscence and left no biographical notes. He was born on 3 April 1887, in Andrychow a small town which was at that time in Silesia but which later became Polish. After attending the local school he was sent to the Gymnasium at Bielsko about 90 miles from his home. His father, Theodor Felix, was a manufacturer with an interest in printed textiles and accordingly it was planned that his son should study dye chemistry. In due course, Felix went to Vienna and studied under Friedlaender. After obtaining his D.Sc. degree for a thesis on indigoid dyes, he returned home to work in his father's factory. However, he wanted to go back to Vienna, since his interest in biological subjects had been aroused during his student days and, in particular, he had come under the influence of Lafar, the mycologist; an additional reason also, may well have been that he had at the same time become very much interested in Zionism. His father was strongly opposed to this wish and

Felix had to remain at home for a time except for a period which he spent in a chemical factory in Holland. Eventually, however, he got his own way, returned to Vienna and took up the study of mycology.

When war broke out in 1914, Felix asked to be assigned to a bacteriological laboratory and was first posted to the military hospital in Wlodowice near his home. Weil, who was in charge of the No. 5 Austrian Mobile Epidemiological Laboratory, came to hear about Felix, arranged for his transfer, and assigned him first to a study of the effects of volatile organic chemicals on lice, since steam disinfestation of clothing was not always practicable in the field. The main purpose of Weil's unit, as of some others, was the investigation of typhus fever. It was known that typhus was a louse-borne disease, but the causal agent had not been identified. There were already indications that this disease 'of war and famine' might cause serious trouble on the Eastern Front, as indeed it did; immense epidemics were to occur in Poland, in Russia and in Serbia. There were difficulties in clinical diagnosis of typhus fever in its early stages and confusion had arisen because blood tests in cases which had been vaccinated against typhoid seemed to indicate that the illness was due to an enteric infection. The question had been raised whether there was another disease clinically resembling typhus fever, but caused by a highly virulent and toxic typhoid-like bacterium. It was in the course of enquiry into this problem that the Weil–Felix reaction was discovered in the autumn of 1915. It was found that a bacillus that had been isolated from a case of typhus was agglutinated not only by the patient's serum but by other typhus sera. This organism, for reasons which will be stated later, was not immediately identified and Weil and Felix at first thought that they had isolated the causal organism of the typhoid-like disease supposed to simulate typhus fever. However, it was soon identified as a member of the saprophytic *Proteus* group and a number of other *Proteus* X strains were isolated. Since these organisms were manifestly not the cause of typhus fever, their relationship to typhus became a first-class immunological puzzle which was only partly solved many years later by Castaneda and Zia when they showed that one of the antigenic substances of typhus rickettsia occurs also in *Proteus* X.

Although the further observations of Weil and Felix and of others seemed to confirm that the presence of agglutinins for *Proetus* X in a patient's blood was diagnostic of typhus, their conclusions met with much criticism and disbelief. In order to obtain further evidence in another area, Felix went to the German Red Cross Military Hospital in Constantinople in October 1916. He rejoined Weil in the following January and soon afterwards they reported that some *Proteus* X strains have two qualitatively different kinds of antigen which they called H and O. They rapidly extended these observations and showed that H and O types of antigen are also present in organisms of the enteric group.

In March 1918, German and Austrian troops occupied the Ukraine and Weil and Felix moved to Kherson near Odessa. Shortly afterwards, rapid changes in the political and military situation caused them hurriedly to abandon their laboratory. They packed up their materials and records, loaded them on cattle trucks and set out for Prague by rail but over two months were to elapse before they got there. Both became ill with an enteric infection and Weil was completely incapacitated most of the time, while Felix guarded the materials night and day. Many times the truck was shunted to a siding for investigation of its passengers and their baggage. The boxes, sometimes suspected of containing ammunition, were nearly confiscated several times. The explanation that they contained cultures from cases of typhus and typhoid undoubtedly saved them from being torn open on the spot, but on one occasion resulted in the trucks being shunted to a lonely siding and left there for two weeks. Eventually they got their precious materials and records to Prague after an epic journey of which, unfortunately, no written account is extant. Before Felix left for Palestine, he and Weil prepared a detailed account of the typhus work they had done together. When the manuscript was completed it was left with Weil to be sent to the publishers. However after Weil's death it was found to have disappeared mysteriously together with all the relevant protocols for the years 1916 to 1920 and all Felix's efforts to locate them were unsuccessful.

The partnership between Weil and Felix continued for a further two years after the end of the war until events provided Felix, who had deep Zionist convictions, with an opportunity to work in Palestine. In 1921 he was appointed Director of the Bacteriological Laboratory of the Hadassah Medical Organization in Tel Aviv and in the following year he became Chief Bacteriologist of the Jerusalem Laboratory. Weil planned to join Felix in Palestine and his death from typhus in 1922 was a severe blow, for not only did Felix have the highest possible regard for Weil's scientific genius but the two men had a very great affection for each other.

Felix remained in Palestine for six years, accomplishing much original work under difficult conditions. His laboratory was poorly equipped, there was much miscellaneous work to be done and he held the responsible post of Chairman of the Health Council in the Palestine Zionist Executive. Although Weil's and Felix's work on *Proteus* X had been widely recognized, their work on enteric organisms had, as Felix complained in 1924, 'remained for a long time unrecognized'. Felix now began to publish in English as well as German and his classic paper on 'Qualitative receptor analysis in its application to typhoid fever' attracted much attention and no little criticism. Among those who became interested in Felix's methods and views were Arkwright and Ledingham, both of the Lister Institute of Preventive Medicine. Following a considerable scientific correspondence, Felix visited them in 1927. Impressed by the sympathetic interest shown in his work and by the contrast in laboratory facilities available in London and in Palestine, he asked to be allowed to come and work in the Lister Institute. This was arranged and he resigned his post in Palestine in November 1927. He was appointed first to the Jenner Memorial Research Studentship and, in April 1931, to the staff of the

Bacteriology Department of the Lister Institute. In London he continued work on the Weil–Felix reaction, but was mainly occupied with problems in the serology, serum treatment and vaccine prophylaxis of enteric infections. In 1934, with the collaboration of Miss S. R. M. Pitt, he discovered the Vi antigen of the typhoid bacillus and in his characteristic way proceeded methodically to work out the details and the practical implications of this discovery.

When war was declared in 1939 he was seconded to the Emergency Public Health Service. From the School of Pathology, Oxford, he was moved in 1940 to the London School of Hygiene. A year later he was transferred to the National Institute for Medical Research, Hampstead, and in November 1942 to the Wellcome Institute, where he remained until the end of the war. His chief contributions during this period were the development of applications in this country of typhoid Vi phage typing and of Vi agglutination tests in suspected chronic carriers. He also discovered some new phage types of *S. typhi* and developed a scheme for the phage typing of *S. para typhi* B. In December 1945 he resigned from the Lister Institute in order to continue in the Public Health Service which was to be created from the 'Emergency' war-time nucleus. He was the unquestionable choice for the Directorship of the Central Enteric Reference Laboratory which was to be organized in the Central Laboratory of the Public Health Service at Colindale in North London. Early in 1946 he visited Canada and the U.S.A. for the main purpose of detailed discussions on enteric phage typing and typhoid vaccine with experts in these countries and, on his return, moved to the Colindale Laboratory.

Felix had devoted much attention to the need for international standardization of the Weil–Felix reaction and enteric agglutination tests. With the development of enteric phage typing, he became equally concerned with the need for international standardization of this method, appreciating clearly the pressing urgency of the problem. Through his intensive efforts the International Committee for Enteric Phage Typing was formed during the Fourth International Congress for Microbiology held in Copenhagen in 1947. He was appointed Joint Chairman and his laboratory in Colindale was appointed the International Reference Laboratory in the scheme adopted. As new countries joined the international scheme for enteric phage typing and the burden of his correspondence increased steadily, his satisfaction was mingled with apprehension for the future of his work when he would have to retire from the Public Health Laboratory Service in the spring of 1953. He was given accommodation in the Lister Institute and there he worked with undiminished determination. His time was divided between the completion of experimental work and acting as Secretary of the International Committee for Enteric Phage Typing. He died suddenly and unexpectedly on 14 January 1956, a few weeks before he was due to retire completely from laboratory work.

Felix's genius lay in an 'infinite capacity for taking pains'. He cultivated a narrow field in his experimental studies, but he did so systematically, tenaciously and intensively. Although in some ways lacking in imagination, he showed an unusual perspicacity in regard to observations which could be developed and applied in the practical fields of diagnosis and prevention. He drew his scientific inspiration from Weil and the greater part of his life's work was built on the foundations they had laid together. The origin of Felix's intensely practical outlook is less certain, but he was undoubtedly greatly influenced by his experiences during the first war. He had no medical training and no practical experience of bacteriology to speak of until he became Weil's assistant. In the four years that followed, his intensive experience of the type of human suffering brought about by epidemic diseases must have deeply affected him. My own impression is that his reserved and enigmatic personality concealed an unwavering conviction of a special mission in life.

Felix was practical and methodical in all that he did. His laboratory work was conducted not only with the greatest care but with the greatest respect for the pathogenic organisms used in it. His records were neatly entered and annotated in variously coloured inks on foolscap sheets ruled in quarter-inch squares. Letters received on technical matters were carefully studied and sentences and words were often marked according to some private colour code. Records and correspondence were meticulously filed. Often, during discussions on enteric phage typing, I have envied the certain and unhurried way in which he quickly assembled, droning quietly and contentedly to himself as he did so, all the relevant protocols and correspondence bearing upon some intricate point. He had a passion for detail and was sometimes unsparing of it when a more concise summary or a more rapid presentation might have served.

He took the greatest pains to ensure the correctness of his observations which he repeated and repeated until he was satisfied, but this did not always guarantee him freedom from errors of interpretation. He formed his opinions slowly and having formed them, held them tenaciously and courageously though sometimes, even in his later years, he was somewhat inclined to be on the defensive and to regard criticism of his interpretation as criticism of his technique and accuracy of observation.

Although Felix's circle of scientific friends and acquaintances was world wide, few knew him with any degree of intimacy. Very far from being unfriendly, he was at the same time reserved and difficult to know even when there was a bond of common scientific interest. He was a familiar figure at the meetings of many societies from all of which he derived a deep enjoyment, for his interests outside his own field were unobtrusive but extensive. He did not lack for humour even if the expression of this was sometimes unrecognized, because so unexpected, and he often had a great deal of amusement from hearing about the researches of others, particularly virologists, of whom he once said: 'Virologists have always been, and still are, rather a greedy species; they swallowed Rickettsia, they conquered bacteriophage, cancer is always in danger of being subjugated by them, and it is difficult to see where they are going to stop.'

The way in which his mind worked was sometimes difficult to follow and sometimes he seemed to find an equal difficulty in understanding the views of others. He was a formidable opponent and, on occasion, could be exasperating and stubborn even with his friends. These, however, forgave him much, for there was something loveable about Felix; he could be extremely charming and kind, capable of great patience and gentleness, and with the capacity of attracting a very real and sincere affection.

In January 1956, Felix had virtually completed the work he was endeavouring to finish before he finally retired from the laboratory bench. Travelling and walking were his main recreations and he was eagerly looking forward to a long holiday abroad. He died suddenly, alone, while on his way back to the Lister Institute one Saturday evening to fetch a reprint. He is survived by his devoted wife and companion, who was Leah Gluckman of Tel Aviv and whom he married in 1923. The tragic death of their only son in 1950 was an exceedingly bitter blow which Felix, outwardly, met with the greatest fortitude.

Felix's work on typhoid was recognized by the award of an honorary degree of D.Sc. from the University of Belfast in 1937. In 1943 he was elected to the Fellowship of the Royal Society and he was made a Foreign Corresponding Member of the Societé de Pathologie Exotique de Paris in 1947.

THE WEIL–FELIX REACTION

The first *Proteus* X strain was isolated on 15 September 1915. Weil and Felix had been posted to East Galicia and were engaged in a bacteriological and serological study of cases of typhus in a hospital there. A Roumanian physician who had been examining the cases on admission, contracted the disease. A Gram-negative bacillus isolated from his urine was found to be agglutinated by his serum and also by sera from other cases of typhus. Initially this organism was non-motile and thought to ferment lactose. Consequently it was not immediately recognized as belonging to the saprophytic *Proteus* group and Weil and Felix at first thought that they had isolated the causal organism of a typhoid-like bacterial disease which had been confused with typhus. The reaction of the organism to typhus serum appeared to be specific and cultures were supplied to neighbouring field hospital laboratories. Weil sent a culture to Bail, his former chief who was Professor of Bacteriology at Prague, and they also sent a preliminary communication to the *Wiener Klinischen Wochenschrift*. Bail tested the culture on typhus sera from the Serbian front and obtained positive reactions. He added a postscript to Weil and Felix's paper in which he referred to the reaction as the Weil–Felix reaction.

Further strains of *Proteus* X were isolated by Weil and Felix and by other workers. At first X2 was used for the Weil–Felix test, but was soon replaced by the more sensitive X19 strain. Although it was obvious that *B. proteus* X was not the causal organism of typhus, some puzzling facts about its relationship with typhus fever came to light. It was isolated from only a small proportion of cases of typhus, but could not be recovered from sources unrelated to the disease. The strains isolated were antigenically distinct from *Proteus* strains from other sources in that they had the OX2 and OX19 antigen which caused them to react with typhus sera. Weil and Felix examined 126 cultures of *B. proteus vulgaris*, including practically all those which existed in 1918 in the collections of universities and institutes in Germany and Austria and none was found to possess the X antigen. On the other hand they found that *Proteus* strains associated with typhus in various parts of Europe and Asia Minor were X strains; 24 being type X2 and 31 type X19. They also examined 225 cases of typhus in the 1915-16 epidemic in Eastern Galicia and all but two were found to have agglutinins for the X2 organism. Later, Felix in a series of 300 typhus cases from Asia Minor found the reaction with X19 positive in all, although 22 per cent were negative with X2.

In view of this and other facts, Weil and Felix in 1921 advanced the hypothesis that in some cases of typhus the causal rickettsia was transformed into *B. proteus* X. This hypothesis was almost universally rejected on the grounds that the X organisms were neither capable of producing typhus nor of inducing immunity thereto. Nevertheless, Felix's belief in the hypothesis of transformation remained unshaken. His last published discussion of the transformation hypothesis was in 1933. It was a disappointment to him that out of the tremendous volume of research which was carried out on typhus during the 1939-45 war, no evidence for the transformation hypothesis was forthcoming.

Although Felix continued his work on typhus fever when he came to London he confined himself to the serological examination of sera from cases of typhus-like diseases from various parts of the world. Despite this restriction, Felix, in his position as the international authority on the Weil–Felix reaction and out of his unrivalled knowledge of the serological complexities and pitfalls, contributed substantially to the classification of the rickettsial diseases of man in the period between the wars. He dealt vigorously with those who claimed that the Weil–Felix reaction was not 'clinically' specific but due to a polyagglutinating quality peculiar to the blood serum of typhus cases. With Miss Rhodes, he confirmed the important observation of Fletcher and his co-workers that the epidemiologically different urban 'shop' typhus and rural 'scrub' typhus were serologically distinct according to their reactions with *B. Proteus* X19 and a variant thereof called the Kingsbury strain.

In 1922, Felix had investigated the antigenic variability of *B. proteus* X *in vitro*. The results of this enquiry were rather alarming and it was suggested that the wide range of variability observed might invalidate the serological diagnosis of typhus. However, no evidence was obtained at that time that the serological variations obtained *in vitro* affected the O antigen shared with rickettsia. Felix exhaustively investigated the K strain of Fletcher and Lessler, satisfied himself that this was a variant derived from the original X19 culture, and concluded that it represented another serological X type. He introduced the symbol XK for the Kingsbury variant and advanced the thesis that the rickettsia of various typhus-like diseases correspond to various types of *B. proteus* X and, further, that serum reactions with the *Proteus* types OX19, OX2 and OXK reflected the relationship obtaining between different typhus rickettsia in regard to cross-immunity. His acceptance of the hypothesis of transformation from rickettsia to *B. proteus* X led him to believe that only diligence was required to isolate the appropriate X strain for the serodiagnosis of each immunologically distinct type of rickettsial infection.

The Weil–Felix reaction provided in most instances the first evidence of the generic relationship between certain 'unclassed' fevers and typhus, thus opening the way to experimental studies and demonstration of their rickettsial nature. In 1935 Felix suggested a scheme of immunological subgroups of typhus-like diseases, type OX19 (epidemic and murine typhus), type OXK (scrub typhus) and undetermined types giving group reactions with OX19, OX2 and OXK.

During the 1939-45 war, Felix served on the Typhus Committee of the Medical Research Council and carried out further work in connexion with the Weil–Felix reaction. Two of his reports were published. One of these deals with 'The typhus group of fevers: classification, laboratory diagnosis, prophylactic inoculation and specific serum treatment.' The other report deals with the technique and interpretation of the Weil–Felix reaction. Although the specificity and immunological importance of the labile antigens of louse-borne and murine typhus rickettsia had been established by North American workers and purified suspensions of rickettsia were available in specialist typhus laboratories for serological tests, the Weil–Felix reaction remained, as Felix insisted, diagnostically useful and had the advantage of giving a positive reaction in the majority of cases of typhus five or six days earlier than rickettsial suspensions. Felix carried out tests of OX19 agglutinin responses in a considerable number of volunteers inoculated with various typhus vaccines and showed that these vaccines differed considerably in this respect. The antigen shared by typhus rickettsia and *B. proteus* X19 is heat stable and Felix drew the inevitable analogy between Vi and O antigens of typhoid and the heat-labile and heat-stable antigens of rickettsia. He argued that the heat-stable antigen, therefore, acted as an endotoxin, that the corresponding OX19 antibody would function as an anti-endotoxin and recommended the use of OX19 immune serum in combination with anti-rickettsial serum for the treatment of louse-borne typhus. A number of horses were immunized to produce a high titre of OX19 serum but no opportunity seems to have arisen for any adequate trial of this.

From time to time Felix was very vexed by the unsatisfactory techniques used by many workers for the Weil–Felix test. The fault really lay with the writers of certain text books. After the second World War, Felix stressed the necessity of standardizing the test and his recommendations for this were published in the *Bulletin of the World Health Organization*, 1950. His suggestion of 'international serum standards' for OX19, OX2 and OXK were accepted by the expert committee on biological standardization of W.H.O.

The great advances made during the 1939-45 war in the cultivation and immunology of the rickettsia and in the prophylaxis and control of typhus in no way diminished Felix's prior contributions. His work provided essential stepping-stones to progress when experimental investigation was difficult, tedious and hazardous.

H AND O ANTIGENS

The following summary of the observations leading to the discovery of H and O antigens is taken from Felix's 1924 paper on 'The qualitative receptor analysis in its application to typhoid fever'.

'In the course of experimental investigations, the purpose of which was to clear up the nature of the diagnostic reaction in typhus fever of Weil and Felix, the following striking fact was, in the first place, to be investigated: the *Proteus*-X-strains (type X2 and type X19) reacted with a serum of typhus cases or convalescents in an entirely specific manner. . . . But they reacted on the one hand, with the serum of patients, and, on the other hand, with artificial rabbit immune serum in an entirely different manner: the serum of patients agglutinated *Proteus* X19 generally many times higher than *Proteus* X2 (10, 20 and even 100 times higher); immune serum of X2 or X19, on the other hand, regularly had the same titer for both types. The agglutination in the serum of the patients resulted relatively slowly and in small, firm flakes; in immune serum, on the other hand, very quickly and in large, loose flakes. Saprophytic *Proteus* strains of the most diverse origins showed strikingly strong agglutination with immune serum (often to the end of the titer); with the serum of patients, on the other hand, they did not react at all. In the serum of patients the agglutinins were almost completely destroyed by heating to 60°C; in immune serum on the other hand, they remained intact. These striking facts formed, for several years, the subject of numerous investigations on the part of many Austrian and German authors. They were at first so incomprehensible and so inconsistent with the existing views of immunology, that they led to the forming of the most varied hypotheses, which were discussed as theories of the Weil–Felix reaction. All these hypotheses have been, it is true, long since refuted by Weil and Felix as entirely untenable, but they are even now cited in literature, in an uncritical manner.

'Weil and Felix succeeded in the year 1917 in solving this complicated problem in the following manner: they ascertained that the X-strains exist in the artificial cultural medium in two forms, which differ from each other morphologically, biologically and serologically and which have been known since in the literature by the name of O-form and H-form.* They represent two forms of variation of the same micro-organism and are mutually transferable into each other. We are concerned here only in the serological differences of these two variants. They can be represented as follows: the immune serum of the O-form (OX19 or OX2) contains only *one* agglutinin, which reacts specifically only with the homologous bacilli and agglutinates them small-flakedly (O-agglutinin): the immune serum of the H-form (HX19 or HX2), contains *two* agglutinins, the specific small-flaking O-agglutinin and a non-specific, large-flaking H-agglutinin, which reacts with the heterologous just as it does with the homologous bacilli. The antigen of the O-forms consists accordingly of *one* type of receptors (O-receptors); the antigen of the H-forms, on the other hand, contains *two* types of receptors (H- and O-receptors).'

It seems strange that Weil and Felix missed the important point, that the spreading character of *B. proteus* colonies '*mit Hauch*' was due to their motility and that the H antigen is associated with flagella. Braun and Schaffer noticed this in 1919 and it became apparent that Weil and Felix had independently arrived at the hypothesis of two antigens, flagellar and somatic, advanced by Smith and Reagh in 1903 for the hog-cholera bacillus. These earlier observations seemed at the time to be of merely academic interest. A decade later, advances in bacterial serology, particularly in the antigenically complex typhoid-salmonella group where many antigens are shared in different ways between the different species, had brought about a state of considerable confusion which was further complicated by the discovery of S-R (smooth-rough) variation. Weil's and Felix's contribution was not so much the discovery of the H and O antigens of *Proteus* and enteric groups, but the fact that their meticulous and painstaking work brought order out of confusion, clarified diagnostic serology and paved the way to improvements in vaccine prophylaxis. It must be admitted that in the earlier stages of their work on the double receptor structure of organisms of the *Proteus* and enteric groups the intricate observations and rather abstract arguments of Weil and Felix were not too easy to follow. I feel, as I did 30 years ago, that Felix's complaint that hardly any attention had been given to their work outside Germany and Austria, was due not only to this but to their reluctance to equate the H antigen with flagella.

QUALITATIVE RECEPTOR ANALYSIS

The widespread use of typhoid and paratyphoid vaccines during the 1914-18 war led to great difficulties in the interpretation of serodiagnostic tests. In persons who had been inoculated at an earlier date, typhoid and paratyphoid agglutinins were apt to reappear in the blood under the non-specific stimulus of a variety of other fevers. For example, Weil and Felix found that a considerable number of persons who had been inoculated against typhoid, showed, during the course of typhus fever, a rise in typhoid agglutinins that previously had been known only in active typhoid infection. The Widal test, which had for so many years been one of the most valuable methods of diagnosis of enteric infections thus lost its value when 'it ceased to become a simple reaction and became a complicated serological problem'. The liveliest controversies arose in the English and German literature regarding the nature of the non-specific stimulation of typhoid agglutinins in other fevers, and over various claims that improved serological methods eliminated the practical difficulties. The problem of the serodiagnosis of enteric fevers in inoculated persons was not solved during the 1914-18 war, and when Felix went to Palestine he was again confronted with it because all immigrants were required to be inoculated with T.A.B. vaccine. Realizing the limitations and ineffectiveness of the universally employed quantitative methods—the titration of serial dilutions of serum against the test organisms—he proceeded systematically to apply the following facts that he and Weil had elicited concerning the double receptor structure of enteric organisms: (1) The labile H antigen is specific for each one of the four species. (2) The stable O antigen consists of main receptors, which are specific for each of these species, and of group receptors which may show cross relationships among themselves. (3) According to this scheme, group agglutination between *S. typhi* and *S. paratyphi* A and B is as a rule caused by small-flaking O group agglutinins. (4) *S. typhi* and *S. enteritidis* possess an identical stable O antigen. These two species differ from each other only in their labile H antigen. (5) Strains of *S. typhi* are not all similar in their sensitivity to O agglutinins.

---

* The symbols 'H' and 'O' are derived from the descriptive terms 'hauchförmig' and 'ohne Hauch' applied to the colonies of *B. proteus* on nutrient agar.

In an elaborate clinical investigation Felix examined 1200 blood specimens from non-inoculated and inoculated persons, some 500 of which came from 250 cases of typhoid or paratyphoid fever. These were tested with cultures of strains of *S. typhi* (2 strains), *S. paratyphi* A and B and *S. enteritidis*. These five strains were carefully selected to simplify the independent estimation of H (large flaking) and O (small flaking) agglutinins. One of the typhoid strains was 901, important because of its extreme sensitivity to O agglutinins. Because of the importance of distinguishing the qualitative differences between H and O types of agglutination, Felix called his method 'qualitative receptor analysis'. Although it did not solve all the problems of the serodiagnosis of enteric fevers, it constituted a considerable advance.

Felix also applied the method in reverse for the identification of organisms of the typhoid-paratyphoid group. For this he used H sera in dilutions beyond their O titre and O sera prepared by inoculating rabbits with cultures heated to 100°C, to destroy the H antigen. He claimed that: 'The qualitative receptor analysis in most instances allows the differentiation of the bacteria of the typhoid and paratyphoid group on the basis of investigation with a single dilution of immune serum.' To many bacteriologists who had grown accustomed to the complexity of quantitative agglutination tests, this idea was revolutionary and, to begin with, unacceptable. Felix was well aware of the antagonism that might be aroused to the qualitative receptor analysis method, for in his 1924 paper he states: 'It is certain that the test of our proofs will also bring many instances of non-confirmation of these results for the techniques of this method require special care and it is liable to create many sources of error if careful attention is not paid to the details described.'

Felix also investigated the relationship between bacteræmia and type of agglutinins present in the blood. He found that large-flaking (H) agglutinins on the one hand and bacteræmia and clinical course on the other hand, showed no regular relationship whatever. However, small-flaking (O) agglutinins did so in one of two ways. When small-flaking agglutinins appeared early, bacteræmia was either not observed at all or found to exist only for a short time and at a low level. Severe or fatal cases developed either very little O agglutinin or none at all. From these relationships he hazarded the opinion that 'the seroprognosis in typhoid fever seemed thereby to have become possible'.

The concluding portion of Felix's important paper on 'The qualitative receptor analysis in its application to typhoid fever' (1924) deals with protective inoculation against typhoid and paratyphoid. He remarked 'to the unbiassed reader whose judgment has not been impaired by the school of opinion current for years, the thought will already have occurred: the above reported results of the application of the qualitative receptor analysis in typhoid fever urgently require a revision of the views concerning the value of preventive inoculation against typhoid and paratyphoid'. He had been unable to demonstrate O agglutinins following antityphoid vaccine and had found them absent or present only in small amount in severe and fatal cases of typhoid. Consequently, 'as antityphoid vaccination, however, causes no changes whatever in the appearance and quantity of these agglutinations in the course of typhoid fever, then no influence can be expected from it on the clinical symptoms.' 'We are well aware of the fact that we shall arouse very great opposition with these conclusions.' 'The facts ascertained by this form of analysis point to the conclusion that antityphoid vaccination, in the form now practised, gives no hope for protective action because it only produces antibodies against the labile antigen, but in order to be efficient it should produce antibodies against the stable antigen. It only remains, therefore, to find a method of vaccination which will fulfil this demand.'

In a further paper on qualitative receptor analysis, Felix and Olitzki (1925) reported the results of bactericidal tests with sera from typhoid convalescents and from immunized animals. H sera, from which O agglutinins had been removed, were found devoid of bactericidal activity. On the other hand, O sera were strongly bactericidal. Further, *S. typhi* and *S. enteritidis*, sharing the same O antigens, possessed the same bactericidal amboceptor; group-amboceptors were found for *S. typhi* and *S. paratyphi* A and B corresponding with the minor O group-agglutinins.

When Felix came to the Lister Institute, he collaborated with Dr Muriel

Robertson in applying the methods of qualitative receptor analysis to the group of the spore-bearing anærobes. They showed, by means of agglutination and agglutinin absorption tests, that *Clostridium septicum* and *Clostridium tetani* possessed H and O antigens analagous to those recognized in ærobic species having a double receptor structure. Failure to demonstrate that the presence or absence of the H antigen had any effect on the intensity of complement fixation confirmed Felix's belief in the ' "purely stabilotrophic" nature of the complement-binding immune body.'

Felix, in observations on which his double receptor hypothesis is based, made a mistake in concluding that the H receptor plays no part in complement fixation reactions. This was unfortunate because it cast serious doubt on the adequacy of some of his techniques: with his usual scrupulous care he had adopted standardized conditions giving adequate and consistent results in complement fixation tests with O antigen and antibody. These, quantitatively, were such that the H reaction was inhibited by prezone effect. Ironically, he fell into a technical trap of his own devizing.

In 1928, he presented a scheme which he described as 'an attempt to formulate a general conception of the bacterial immune bodies'. According to this double receptor hypothesis, the labile antigen (H receptor) participates only in agglutination and precipitin reactions; the effective agent in complement fixation, as well as bactericidal and opsonic serum effects, consists purely of the interaction of the stable O antigen and the O or stabilotropic immune body. 'One serum body; and one only, namely the stabilotropic immune body, seems to be involved as the sole important factor in those phenomena of humoral immunity specified above.' This statement is followed by the remark 'this hypothesis will, of course, meet with much criticism'. Indeed, the climate of opinion in some quarters was not well disposed towards Felix's ideas or methods. Practical bacteriologists were endeavouring to become oriented with much new, but incomplete and, therefore, confusing, knowledge about serological variation, such as smooth-rough variation, diphasic variation of flagellar antigens and the serological complexity of the *Salmonella* group.

The acceptance of Felix's methods as a practical tool for the serodiagnosis of enteric fevers was undoubtedly hindered by the terminology he used and by his initial insistence on the use of freshly prepared suspensions of live pathogenic organisms. It had long been the practice to use, for diagnostic purposes, bacillary suspensions killed and preserved in bulk by means of formalin or phenol. Although such suspensions retained their H agglutinability they were quite unsuitable for the demonstration of O antibodies. However, in 1917, Bien and Sonntag had shown that an alcohol-treated suspension of *B. proteus* X was suitable for the diagnosis of typhus. Later, Felix and Olitzki investigated this method of preservation and were unable to demonstrate any adverse effects on O agglutination. They, therefore, adopted the suggestion of Schiff (1925) that two differently preserved suspensions of each organism should be used in the routine diagnosis of enteric fever, namely, (1) an alcoholic suspension for O agglutination and (2) the usual phenol or formalin suspension for H agglutination.

In 1930, in a brief and lucid account of the technique of qualitative serum diagnosis of enteric fevers, Felix wrote that 'this method has been alluded to as "somewhat of a mystery". While this statement does not appear to be justifiable, it must be admitted that the new method is unknown to clinical pathologists.' Actually, when these words were written the attitude of indifference or criticism to which Felix was so sensitive was definitely on the wane. The first edition of Topley and Wilson's *Principles of bacteriology and immunity* (1929) included an excellent account of what was then known of the occurrence of H and O antigens in the *Salmonella* group. Early in 1930, Gardner, Hobson and Stenhouse demonstrated, for the first time in this country, the value of the O agglutination test in a case of typhoid with a persistent negative Widal reaction.

In 1949, the World Health Organization Committee on Biological Standardization accepted Felix's proposal to prepare eight horse antisera for use in the standardization of agglutinable suspensions employed in the serological diagnosis of typhoid and paratyphoid infections. Sera were prepared in horses in co-operation with the serum department of the Lister Institute of Preventive Medicine at Elstree and dried in ampoules in the Department of Biological Standards, National Institute for Medical Research, London. In 1954, Felix and Bensted reported on seven of these dried standard agglutinating sera and made proposals for their further examination on an international basis and for their future use. These standards comprised sera for typhoid (H, O, and Vi), paratyphoid-A (H) and paratyphoid-B (specific H, non-specific H and O) agglutination. It was recommended that these international standards be issued only to approved national laboratories for biological standards to enable them to prepare and distribute Standard Agglutinating Sera based on the corresponding international standards and to issue agglutinable suspensions tested against the corresponding Standard Agglutinating Sera.

THE VI ANTIGEN OF *S. typhi*

In 1934, Felix demonstrated by *in vitro* tests, a new antigen peculiar to virulent strains of *S. typhi*. The recognition of this Vi antigen was unexpected because of the vast amount of serological work that had preceded it. Actually most of this work was irrelevant, but some early clues had been missed. It was well known by 1904 that freshly isolated strains of *S. typhi* were sometimes inagglutinable in rabbit antiserum prepared against a stock reference culture. In 1920 Weil and Felix recorded striking differences in the agglutinability of different strains of *S. typhi* by anti-O sera. Felix selected strain 901 for use in his qualitative receptor analysis method because of its remarkable sensitivity to O agglutination and strain Ty2 for its high resistance. Later, with Olitzki, he noted that such highly sensitive strains were also so highly susceptible to the bactericidal action of normal serum that they could not be used in bactericidal tests.

In 1930 Grinnell reported on the results of comparative bactericidal tests with the blood of human beings before and after receiving the customary course of prophylactic inoculations with a vaccine prepared from a smooth, freshly isolated strain of *S. typhi* and with a vaccine made from the Rawlings strain. The bactericidal power of the blood of those receiving the smooth vaccine showed a considerable increase, that of those receiving the Rawlings vaccine was, if anything, slightly decreased. The Rawlings strain had been isolated in 1903, but although it had become increasingly difficult to maintain in the smooth form, the British vaccine and most of the U.S.A. and Canadian ones were still being prepared from it. Grinnell (1937) next undertook a study of Rawlings strains from 18 different U.S.A. laboratories engaged in making antityphoid vaccine. He developed a mouse test for the virulence of different strains and for assessing the immunizing potency of vaccines. These tests depended on the development of infection and toxæmia and death within two days in mice inoculated intraperitoneally with rather large doses of fresh culture. At the same dose level the recently isolated strain killed 38 out of 40 while the Rawlings strains killed only two out of 60 mice inoculated. The results in mice receiving vaccines were striking; only the vaccine prepared from the freshly isolated strain gave significant protection. Perry, Findlay and Bensted, of the Pathological Department of the Royal Army Medical College, quickly confirmed and extended Grinnell's observations. They succeeded in rejuvenating their Rawlings strain by selective mouse passage so that it became highly virulent and highly efficient as a vaccine for mice. They also showed that recently isolated strains of *S. typhi* differed considerably in virulence.

It then occurred to Felix that the difference in virulence of strains of *S. typhi* such as had been observed by Perry, Findlay and Bensted, might be due to difference in serum sensitivity and that virulent strains should be resistant to O agglutination. Using a number of selected strains, Felix and Pitt in 1934 reported that the virulence of these for the mouse was closely associated with their resistance to O agglutination at 37°C. Inagglutinable strains such as Ty2, Watson and Giglioli, were highly virulent whereas the highly agglutinable 901 and degraded Rawlings strains were of low virulence. Felix and Pitt interpreted their results in terms of natural O antibody; 'it seems justifiable to assume that the natural resistance to infection with this type of organism depends on the presence, in the invaded host, of an adequate quantity of this antibody'. They found that the agglutinability of virulent strains was annulled when the organisms were killed by heat or alcohol and concluded that some 'obscure property' is required to render the O antigen resistant to O agglutination. They also vaccinated mice with heat-killed suspensions of the highly virulent strains Ty2 and Giglioli and the agglutinable strains Ty8 and Rawlings and then tested for immunity by intraperitoneal inoculation with a lethal dose of a virulent Ty2 culture. The clear-cut results seemed to indicate that the virulent inagglutinable strains possessed an antigen which stimulated a powerful protective antibody. However, there were difficulties in reconciling this interpretation with other observations. All efforts to demonstrate an antigenic difference between virulent and non-virulent strains by agglutination and agglutinin absorption tests had failed. Further, as already mentioned, it had been found that when virulent organisms were killed by heat their inagglutinability was annulled, yet vaccines so prepared protected against an otherwise fatal dose of living virulent organisms. It therefore seemed that the protective antigen, already indicated by the work of Perry, Findlay and Bensted, was not demonstrable by *in vitro* tests. It was at this point that Felix proceeded to immunize rabbits with living cultures. On analyzing their serum by agglutin and agglutinin absorption tests, he found that those injected with virulent cultures developed, in addition to H and O agglutinins, another antibody which agglutinated the O resistant virulent strains. Felix showed that the corresponding antigen was the ‘obscure property’ which rendered living virulent cultures resistant to O agglutination by O antiserum and called it the Vi or virulence antigen. This new antigen which had eluded Felix for 15 years in his careful serological studies of typhoid and caused him to attach exclusive immunological significance to the O antigen came, as he admitted, ‘as somewhat of a surprise’.

In his preliminary communication on the Vi antigen, Felix not only showed that Vi antibody agglutinated virulent O resistant strains, but also reported other experiments indicating the importance of this antibody in passive protection tests. He also confirmed the findings of Perry, Findlay and Bensted that more virulent strains made more efficient vaccines as judged by mouse tests.

The discovery of the Vi antigen was an enormous advance. It placed in the hands of investigators all over the world a simple agglutination test which could be immediately exploited in the investigation of epidemiological problems of typhoid. The fact that this antigen was to prove complex in its character and require a great deal of intricate investigation did not impede such practical applications, but did delay the fulfilment of Felix’s hopes for effective serum therapy and a typhoid vaccine of unquestioned superiority.

As soon as the immunological significance of the Vi antigen became apparent, steps were taken in collaboration with G. F. Petrie to produce Vi + O antiserum in horses for the treatment of typhoid cases. Two horses were given a preliminary course of heat-killed bacilli to prepare them for the final course of living organisms. However, Felix found that high titre Vi agglutinating sera could be obtained in rabbits by a course of injection of formolized extracts of Vi cultures and thus it seemed that the difficulties and risks foreseen in the injection of virulent living typhoid bacilli into horses, might be avoided by giving the horses a second course of formolized extracts. This hope had to be abandoned when further experimental work by Felix and Bhatnagar showed that the Vi antibody elaborated in response to formolized

Vi antigen in rabbits is not identical with that obtained from immunization with 'natural' Vi antigen contained in living bacilli. The antiserum produced by formolized preparations was found to be highly deficient in phagocytosis-promoting activity and in power to protect mice against virulent strains of typhoid. Accordingly, the two horses received a final course of living virulent bacilli. One became ill with continued fever and fully confirmed previous fears of the risks involved by passing virulent typhoid bacilli in its urine. This difficulty was overcome by applying the discovery of Felix and Pitt of rough Vi variants derived from virulent strains, which, lacking the O antigen, were non-virulent. However, technical difficulties inherent in the use of living cultures remained. Numerous attempts were made to discover a method of preparing a sterile antigen capable of replacing the 'natural' Vi antigen of living typhoid bacilli. These included an alcohol-treated vaccine and the antigenic fractions prepared by Topley and Raistrick and by Henderson and Morgan respectively. All these attempts failed because sufficiently high titres of Vi antibody could not be obtained. The method finally adopted was to immunize horses with alcohol-killed vaccine and select those with the highest agglutinin titres for a rapid course of further immunization with a living rough Vi variant.

The preliminary clinical trials of the new Vi + O antityphoid serum were carried out in the autumn of 1934 in Palestine, under the auspices of the Colonial Office. A series of 34 cases, ranging from mild to very severe, was treated. The moderately severe and severe cases responded in a manner suggesting a marked antitoxic effect due, presumably, to the O antibody. On the other hand, none of the cases showed improvement attributable to the Vi antibody, but few were treated early enough to benefit from the expected anti-invasive function of this antibody.

Further laboratory investigations by Felix and his colleagues showed that although virulence depended on the combined presence of Vi and O antigens, Vi antibody alone was sufficient to confer passive protection in mice. A joint investigation with the Palestine Department of Health showed that recently isolated strains of typhoid contained Vi antigen and also that Vi antibody was not readily elaborated as a result of an attack of typhoid fever. Felix, therefore, persevered towards his objective of an antityphoid serum containing an effective amount of Vi antibody as well as O antibody and devoted much careful effort to the problems of laboratory testing and standardization of typhoid serum. There was no immediate opportunity for an adequate controlled clinical trial of this serum and the war intervened. Some favourable reports on the serum appeared, but opinion was divided regarding its value. It held its own against sulphonamides and penicillin, both of which proved ineffective in the treatment of typhoid, but when the effectiveness of chloramphenicol was established, the routine use of antityphoid serum was abandoned.

In his first paper on the Vi antigen, Felix discussed its significance as regards typhoid immunization and indicated that preliminary investigations on its stability and other properties would have to precede the development of a more efficient typhoid vaccine. When it was found that alcohol-killed typhoid bacilli stimulated in the rabbit abundant Vi antibody of full protecting and opsonizing power, an experiment was arranged to test the antibody response to such a vaccine in the human subject. The results were encouraging but disappointment followed when samples were tested in rabbits six months later and it was found that their capacity to stimulate Vi antibody was almost entirely lost. The problem of producing a stable Vi vaccine suitable for routine use was not solved until Wilson suggested that instead of removing the alcohol and adding one of the usual preservatives employed to keep vaccines sterile, alcohol might be used as a preservative. This suggestion was soon found to be a most valuable one and in 1941 Felix described the preparation of alcohol-killed and alcohol-preserved T.A.B.C. vaccine, and with Retford and Stokes, reported on the antibody response and systemic reactions to it in human subjects. The new type of vaccine was first used by the Emergency Public Health Laboratory Service for the inoculation of civilians; it was adopted by the Royal Air Force in 1943 and by the Army in 1944. In November, 1949 Felix and Anderson started tests on three of the vaccines made in February 1940 and kept at 1 to 2°C, and they found that their power to stimulate Vi and O antibody was unimpaired. The stability of the vaccine having thus been demonstrated, Felix made recommendations for 'The preparation, testing and standardization of typhoid vaccines'. In this paper he discussed the most important sources of error in mouse protection tests and severely criticized the use of the mucin technique. He offered suggestions for routine examination in the course of preparation of the vaccine and the efficient control of the potency of typhoid vaccine and recommended standardization of this potency in terms either of an alcohol-preserved or dried vaccine serving as 'standard vaccine'. The assessment of the immunizing potency of typhoid vaccines and the comparison of different typhoid vaccines still presents great difficulties. Typhoid fever cannot be reproduced in experimental animals and its epidemiological features make adequate comparative field trials an almost unattainable ideal. Those which have been carried out have neither shown that alcoholized vaccine is strikingly superior nor inferior to other kinds. Felix's contention that alcohol-treated vaccine is superior to ordinary heat-killed phenolized vaccine is based on its capacity to stimulate development of circulating Vi antibody in the rabbit and man and on his conclusion that the main biological function of the Vi antigen 'is to protect the O antigen against the action of the natural or immune O antibody, thereby protecting the bacterial cell against phagocytosis and the bactericidal action of serum'.

Felix, Krikorian and Reitler in an investigation undertaken as a corollary to the first clinical trials of the new antityphoid serum in Palestine, found that Vi antibody was not readily elaborated as a result of an attack of typhoid fever and was, therefore, of little diagnostic value in typhoid patients. Observing, however, a significant titre of Vi antibody in the blood serum from a typhoid carrier they suggested that Vi agglutination tests might be useful in the detection of typhoid carriers. Bacteriologically, this is often a difficult task, particularly when excretion of *S. typhi* is intermittent, and the value of the suggested serological test was investigated by many workers abroad, most of whom found that the frequency of a positive Vi reaction in chronic typhoid carriers ranged from 25 to 100 per cent. Despite the fact that this test may be negative in some chronic carriers and may occasionally yield a false-positive reaction in non-excretors of *S. typhi* or individuals harbouring a Vi strain of *Bact. coli*, it provides a useful screening test for carrier suspects. It is also useful in detecting in convalescence individuals who may pass into the chronic carrier state. In 1945 the Vi testing of recovered typhoid patients became a routine procedure in Britain. Together with typhoid phage typing, this test has advanced the control of enteric fevers by simplifying the detection of the chronic carrier.

Earlier difficulties with the Vi agglutination test were due to the fact that it requires much greater care than the H or O agglutination tests, but the technique was much simplified with the introduction by Felix of preserved suspensions and also by the discovery by Bhatnager of strain Vi 1 which is a pure reagent for the demonstration of typhoid Vi agglutinins of a standard agglutinating T Vi serum and a standard agglutinable T Vi suspension. Felix then worked to prepare the way for the necessary standardization of this and other diagnostic agglutination tests by international agreement, the key to this standardization being the 'international serum standards' referred to on p. 66.

OTHER VI ANTIGENS

It was inevitable that Felix should search, at the earliest possible opportunity, for 'Vi antigens' in paratyphoid organisms. In 1936, with Pitt he reported the presence of labile somatic antigens in *S. paratyphi* A and *S. paratyphi* B which they regarded as analogous to that of *S. typhi*. That of *S. paratyphi* A was specific, but that of *S. paratyphi* B and *S. typhi murium* were identical. Just before this Kauffman had discovered that the Vi antigen of *S. typhi* occurred in some strains of *S. paratyphi* C and, a few years later, even in some strains of *Bact. coli*. Felix found that the demonstration of the new 'Vi antigens' was difficult because the effects of their presence on virulence, O-inagglutinability and immunological properties are less conspicuous than those of the Vi antigen of *S. typhi*. He, therefore, attempted to characterize these antigens further in terms of their lability to various chemical and physical agents.

Initially Kauffman was in favour of accepting such labile somatic antigens as a new class of antigens for incorporation into the Kauffman–White Schema of *Salmonella* antigens. However, he soon found that the 'Vi antigen' of *S. paratyphi* B was the 'O-factor V' of the Kauffman–White Schema. Later, as his intensive work on the diagnostic serology of the *Salmonella* and *Coli* groups revealed a still greater complexity of antigenic structure he introduced new designations and finally created a new K series of surface and capsular antigens which included the typhoid Vi antigen, the M (mucoid) antigens of *Salmonella* species and the L, A, and B antigens of *Bact. coli.* Kauffman considered this justified; he did not feel it necessary to subdivide the K antigens for the practical purpose of the diagnostic Kauffman–White Schema. Felix was convinced that the typhoid Vi antigen was the prototype of a series of antigens presented in *Salmonella* and other Enterobacteriaceae. He vigorously rejected and attacked Kauffman's classification, for the relegation of the typhoid Vi antigen to a position of minor importance in Kauffman's classification wounded him deeply.

ENTERIC PHAGE TYPING

The discovery of the Vi antigen of *S. typhi* raised the question of whether typhoid fever was invariably due to infection with the Vi form. Investigations in various countries showed that with few exceptions all freshly isolated strains were Vi strains, although some might degrade rapidly. All of these, irrespective of source, were serologically identical; in brief, there appeared to be only one kind of virulent typhoid bacillus. As a result of the increased attention given to the Vi testing of strains immediately on isolation, bacteriophages which specifically attack Vi cultures, but are inactive in the absence of the Vi antigen, were discovered independently in Canada, France and Holland in 1936. With Yen, I showed that my Canadian strains of Vi phage could be divided into four distinct groups, differing serologically and in other respects. One of these, a type II phage, gave us a good deal of trouble until it was realized that its inconsistent behaviour on propagation was due to an unusual ability to adapt itself to the strain of *S. typhi* on which it was being grown. For example, if grown on Ty2, the phage obtained was some 10^6 times more active against Ty2 than Rawlings. When grown on Rawlings, the ratio of activities was reversed. Additional adapted preparations were made for other strains having a high relative resistance to the Ty2 and Rawlings phage preparations. By suitably diluting these potent phage preparations a set of working preparations was obtained, with which it was possible, by means of a simple plate technique, to identify Vi strains similar to the strain to which the phage had been adapted.

It was soon established that strains which are epidemiologically related are of the same phage type. By 1940, 18 types and subtypes of *S. typhi* had been identified by means of adapted preparations of the original Vi phage II, and this phage typing method applied to the epidemiological investigation of numerous outbreaks of typhoid in North America and elsewhere.

In the early stages of the development of typhoid phage typing Felix did not attempt to use the method although he showed considerable interest. In 1939 it was discovered by others that while most of his cultures of the Watson strain were type F, as they should have been, some were type A. Felix, understandably, was rather perturbed about this because either some of the cultures had changed, unknown to him, or a mistake in identity had been made some years earlier. Fortunately, soon afterwards another observation, the significance of which was unknown to me at the time, more than made amends. Like many others I had assumed that Felix had isolated the classical strains, Ty2 and 901, in Palestine. Over the years Ty2 had remained in the Vi form. On the other hand, 901 was an O-agglutinable strain from the time of first examination and cultures remained in the non-Vi state for 18 years until, in 1936, Kauffman succeeded in recovering the Vi form. This enabled me to type 901 and I found it similar to Ty2. When Felix learned that both these strains were type E1 strains, he wrote to tell me that Ty2 and 901 were isolated from the same epidemic in Kherson, Russia, in 1918.

When war broke out Felix was transferred to the Emergency Public Health Service and I supplied him with my standard phage preparations and reference strains of *S. typhi*. Arrangements were made for all cultures of typhoid isolated in the constituent laboratories of the Emergency Public Health Service to be submitted to him for typing. In Britain the situation was somewhat different from some other parts of the world in which the method had been applied, for typhoid here had become less common, although endemic foci of unknown number, maintained by chronic carriers, remained a menace. Numerically, therefore, the scope for typhoid Vi phage typing was not very great. However, by 1943, Felix had typed 440 strains of typhoid from cases and carriers and had discovered four new phage types. In some instances his findings were followed by special epidemiological investigations conducted by the Ministry of Health. One of the most interesting of these started with the recognition of a new type (D4) in a solitary case. During the following two years a further 19 cases of type D4 infection occurred scattered over four counties and ten administrative districts. Despite the distribution of these cases in time and place, the fact that they were due to the same unusual type of *S. typhi* indicated a high probability that they had a common origin. This, together with some evidence pointing to milk as a vehicle, finally led, step by step, to a farm in Wiltshire and to a milker carrying type D4.

During 1941-43, when large scale outbreaks of paratyphoid B fever occurred in Great Britain, Felix and Callow isolated and screened a considerable number of para B phages. One of these appeared adaptable and they developed a phage typing scheme applicable to strains isolated from cases and carriers. At first these phages appeared to be as specific as the Vi phages of *S. typhi* and their action was assumed to be dependent upon the presence of the B Vi antigen. However, it was shown that the B Vi antigen is identical with the O-factor V of the Kauffman–White Schema and that the B Vi phages lyse some other *Salmonella* species lacking the B Vi antigen. When cross-neutralization tests were carried out, it was found that only three out of seven 'adapted' phage preparations were actually descendants or mutants of their original type I para B phage, the others being 'substitutes' derived from cultures carrying a 'natural' phage. The paratyphoid B phage typing scheme is, therefore, in large part a 'pattern' scheme and utilizes three sero-distinct kinds of bacteriophage. It thus departs in principle from the typhoid scheme in which only phenotypic or genotypic mutants, serologically indistinguishable from the original No. 6 typhoid II Vi phage preparation, are employed. Despite the limitations of the paratyphoid B scheme, the stability and epidemiological significance of the types it distinguishes is of a high order and many workers in different countries have confirmed its reliability and usefulness.

Felix and Callow also attempted to isolate and adapt a bacteriophage for the typing of *S. typhi murium*. Initially Felix considered his scheme superior to that of Lilleengen, in which randomly collected O bacteriophages were used. He was confirmed in his opinion by the results of comparative tests which they carried out together in the Colindale Laboratory. The two sets of results cut across each other and the logical conclusion seemed to be that both could not be of the same epidemiological significance. Later Felix conceded that this was wrong and that it was possible to identify *S. typhi murium* strains from the same focus of infection by means of two entirely different series of bacteriophages which possess different lytic spectra and yield entirely different groupings of the strains.

In 1945, Felix visited Toronto and we discussed in detail all our observations on the phage typing of *S. typhi*. The matter that concerned us most was the need for international standardization. We both feared that the adaptability of typhoid II Vi phage, on which the typing scheme depended, could become the greatest menace to international exploitation of the method if inexperienced workers prepared their own supplies of typing phages from the reference strains supplied. Some, in so doing, had obtained preparations of modified specificity which their own limited tests failed to reveal. On one occasion this resulted in different type designations being allocated to strains from the same epidemic in two different states.

The problem was in fact much simpler than that of international standardization of sera or agglutinable suspensions for serodiagnostic tests; matters in which Felix had had much experience. Because of the stability of the type II Vi phage and the ease with which sufficient quantities of highly active standard phage preparations could be made, it was possible to prepare stocks which would serve not merely as reference standards, but as actual working reagents for a considerable number of years. I, therefore, proceeded to prepare and test by comparative differential titration a new set of typing phages starting with my original Vi phage II preparation and Felix did the same for his seven new types. These phage preparations were of such activity that, in some instances, the 100 ml. volumes prepared were estimated to be sufficient, disregarding inevitable wastage, for more than 10^8 separate tests. Together, in 1947, we published suggestions for the standardization of phage typing of *S. typhi* and recommended a typing scheme designed to accommodate new types which might be found later.

It would be impossible to pay too high a tribute to the part played by Felix in the developments that followed. With characteristic thoroughness he considered every point about our proposed scheme for the international standardization of enteric phage typing and arranged for a committee of experts to meet in Copenhagen during the IVth International Congress for Microbiology in July, 1947. The recommendations were adopted, the International Committee for Enteric Phage Typing was formed, and Felix's laboratory became the International Reference Laboratory, serving national laboratories in nine countries. By this time I had become engaged on work in another field and the burden of further development fell entirely on Felix. The work of the International Reference Laboratory increased. He went to infinite trouble in getting enteric phage typing organized in other countries and in helping workers in their initial technical difficulties. He received many visiting workers from abroad for training and his correspondence on technical matters grew to formidable dimensions. At the Rome Congress in 1953, he had the satisfaction of reporting that the total number of national and regional laboratories for enteric phage typing was 56, and was able to submit a comprehensive world survey of typhoid and para B types based on data received from countries participating in the scheme (published 1955).

When Felix retired from the Emergency Public Health Service he was provided with accommodation in the Lister Institute for Preventive Medicine where he continued laboratory work, partly devoted to completion of earlier unfinished investigations and partly to problems of phage typing. The Colindale Laboratory remained the International Reference Laboratory for Enteric Phage Typing, but Felix undertook to act as Secretary of the International Committee for Enteric Phage Typing and the work involved, together with his other activities, kept him fully occupied until the end.

J. CRAIGIE

BIBLIOGRAPHY

1909. (With P. FRIEDLAENDER.) Über indigoide Farbstoffe (Aliphatisch-aromatische Verbindungen). *S.B. Akad. Wiss. Wien.* **118**, Abt. IIb, 727.

1915. Zur Methodik der Läusevertilgung durch Dämpfe chemischer Agenzien. *Wien. klin. Wochenschr.* **28**, No. 24.

1916. (With E. WEIL.) Zur serologischen Diagnose des Fleckfiebers. *Wien. klin. Wochenschr.* **29**, No. 2.

1916. (With E. WEIL.) Serologische Fleckfieberdiagnose. *Feldärztl. Blätter der k.u.k.2. Armee*, No. 5.

1916. (With E. WEIL.) Die Kurve der Fleckfieberagglutination und ihre praktische Verwertung. *Feldärztl. Blätter der k.u.k.2 Armee*, No. 11.

1916. Serologische Untersuchungen an Fleckfieberkontaktpersonen. *Feldärztl. Blätter der k.u.k.2 Armee*, No. 11.

1916. (With E. Weil.) Erfahrungen mit der Weil-Felixschen Flecktyphusreaktion. *Feldärztl. Blätter der k.u.k.2. Armee*, No. 17.

1916. Die Serodiagnostik des Fleckfiebers. *Wien. klin. Wochenschr.* **29**, No. 28.

1916. (With E. Weil.) Ueber die Beziehungen der Gruber-Widalschen Reaktion zum Fleckfieber. *Wien. klin. Wochenschr.* **29**, No. 31.

1917. (With E. Weil.) Untersuchungen ueber das Wesen der Fleckfieber-Agglutination. *Wien. klin. Wochenschr.* **30**, No. 13.

1917. Serologische Untersuchungen an Fleckfieberkranken aus der asiatischen Türkei. *Z. Immunitätsf.* **26**, 602-619.

1917. Ueber die Züchtung der spezifischen Proteus-(X)-Stämme bei Fleckfieber. *Münch. med. Wochenschr.* **64**, 1259-1261.

1917. (With E. Weil.) Weitere Untersuchungen über das Wesen der Fleckfieber-Agglutination. *Wien. klin. Wochenschr.* **30**, No. 48.

1917. Ueber die Permeabilität der Meningen bei Fleckfieber (Weil-Kafkasche Hämolysinreaktion). *Med. Klin., Berl.* **13**, No. 18.

1918. (With E. Weil.) Merkblatt zur serologischen Fleckfieber diagnose nach Weil-Felix. *Munch. med. Wochenschr.* **65**, No. 1.

1918. Ueber die angleblichen polyagglutinatorischen Eigenschaften des Serums Fleckfieberkranker. *Wien. klin. Wochenschr.* **31**, No. 1.

1918. (With E. Weil.) Untersuchungen über die gewöhnlichen Proteus-Stämme und ihre Beziehungen zu den X-Stämmen. *Wien. klin. Wochenschr.* **31**, No. 23.

1918. (With F. Mitzenmacher.) Weitere Untersuchungen über den Nachweis der O-und H-Rezeptoren bei den Proteus-Stämmen. *Wien. klin. Wochenschr.* **31**, No. 36.

1918. (With E. Weil.) Ueber die Doppelnatur der Rezeptoren beim Paratyphus B. *Wien. klin. Wochenschr.* **31**, No. 36.

1918. (With E. Weil.) Zur Frage der Spezifität der X-Stämme und der Weil-Felixschen Agglutination bei Fleckfieber. *Wien. klin. Wochenschr.* **31**, No. 43.

1918. (With E. Weil and E. Mitzenmacher.) Ueber die Doppelnatur der Rezeptoren in der Typhus-Paratyphus-Gruppe. *Wien. klin. Wochenschr.* **31**, No. 46.

1920. (With E. Weil.) Ueber den Doppeltypus der Rezeptoren in der Typhus-Paratyphus-Gruppe. *Z. Immunitätsf.* **29**, 24-91.

1920. (With E. Weil.) Serologische Untersuchung von Kaninchen nach Behandlung mit Fleckfiebervirus. *Wien. klin. Wochenschr.* **33**, No. 20.

1920. (With E. Weil.) Ueber die Bedingungen der Agglutininbildung durch das Fleckfiebervirus. *Wien. klin. Wochenschr.* **33**, No. 30.

1920. (With E. Weil.) Ueber das Verweilen des Fleckfiebervirus im Meerschweinchenorganismus. *Wien. klin. Wochenschr.* **33**, No. 36.

1921. (With E. Weil.) Ueber die Beziehungen der Fleckfieberagglutination zum Fleckfiebererreger. *Z. Immunitätsf.* **31**, 457-512.

1922. Ueber Varianten der Proteus X-Stämme. *Z. Immunitätsf.* **35**, 57-96.

1924. Ueber die Bedeutung der Rezeptorenanalyse für die Serodiagnostik der typhösen Erkrankungen. *Z. Immunitätsf.* **39**, 127-170.

1924. The qualitative receptor analysis in its application to typhoid fever. *J. Immunol.* **9**, 115-192.

1924. Nissionoth leanalysis ekhuthith shel hareceptorim mikwutzath hatyphus (Qualitative receptor analysis in the typhoid group.) *Harefuah*, **1**, 33.

1924. (With R. Yunowich.) The importance of polyvalence in the use of therapeutic antimeningococcus serum. *J. Immunol.* **9**, 193-206.

1924. Nuovi risultati nella sierologia del tifo addominale. *Bull. Scienze Mediche, Bologna*, Anno XCVI (Serie X), **2**, 113-122.

1925. (With L. Olitzki.) Serumbactericidie und Qualitative Rezeptorenanalyse. *Klin. Wochenschr.* **4**, 72.

1925. (With L. Olitzki.) Pe'ulah baktericidith shel nissyov hadam ve' analys ekhuti shel hamekablim. (Bactericidal serum action and qualitative receptor analysis). *Harefuah*, **1**, 145.

1925. Observations on vesical bilharziasis in Palestine. *Amer. J. Trop. Med.* **5**, 41-61.

1925. Hookworm in Palestine. *Amer. J. Trop. Med.* **5**, 291-296.

1925. Ha'aroth lema'amro shel Dr Malhi: Shnei mikrei Schistosomum mansoni b'eretz Israel. (Comments on Dr Malhi's paper on two cases of schistosomum mansoni in Palestine). *Harefuah*. **1**, 151.

1925. Mahlath Tholaei-vav (Ankylostomiasis) be'eretz-Israel (Hookworm in Palestine). *Harefuah*, **1**, 207.

1926. (With L. Olitzki.) The qualitative receptor analysis. II. Bactericidal serum action and qualitative receptor analysis. *J. Immunol.* **11**, 31-80.

1926. The epidemiology of typhoid fever in Palestine. *Briuth. Haam. Jerusalem*, **1**, 5. (Hebrew.)

1928. (With M. Robertson.) Serological studies in the group of the spore-bearing anaerobes. I. The qualitative analysis of the bacterial antigens of *B. oedematis maligni* (*Vibrion septique*) and *B. tetani*. *Brit. J. Exp. Path.* **9**, 6-18.

1928. (With L. Olitzki.) The use of preserved bacterial suspensions for the agglutination test. With especial reference to the enteric fevers and typhus fever. *J. Hyg.* **28**, 55-66.

1929. On the non-specific stimulation of agglutinins. With especial reference to the enteric fevers and typhus fever. *J. Hyg.* **28**, 418-448.

1929. (With L. Olitzki.) The effect of heat on O and H immune bodies. *Brit. J. Exp. Path.* **10**, 26-34.

1930. The qualitative serum diagnosis of enteric fevers. Directions for its performance. *Lancet*, **i**, 505-506.

1930. (With M. Robertson.) Serological studies in the group of the spore-bearing anaerobes. II. *In vivo* experiments with an 'O' immune serum to *Vibrion septique* devoid of antitoxin content. *Brit. J. Exp. Path.* **11**, 14-23.

1930. Typhus fever (Section B: Serological diagnosis). In *A system of bacteriology in relation to medicine*. Med. Res. Council, H.M. Stationery Office, London, **7**, 412-432.

1931. (With M. Rhodes.) Serological varieties of typhus fever. *J. Hyg.* **31**, 225-246.

1931. Specific and non-specific serum reactions in typhus fever. *J. Hyg.* **31**, 382-402.

1933. The rabbit as experimental animal in the study of the typhus group of viruses. *Trans. Roy. Soc. Trop. Med. & Hyg.* **26**, 365-378.

1933. Serological types of typhus virus and corresponding types of *Proteus*. *Trans. Roy. Soc. Trop. Med. & Hyg.* **27**, 147-172.

1934. (With R. M. Pitt.) Virulence of *B. typhosus* and resistance to O antibody. *J. Path. Bact.* **38**, 409-420.

1934. (With R. M. Pitt.) A new antigen of *B. typhosus*. Its relation to virulence and to active and passive immunisation. *Lancet*, **ii**, 186-191.

1934. (With S. S. Bhatnagar and R. M. Pitt.) Observations on the properties of the Vi antigen of *B. typhosus*. *Brit. J. Exp. Path.* **15**, 346-354.

1935. Clinical trials with a new antityphoid serum. *Lancet*, **i**, 799-802.

1935. The serology of the typhus group of diseases. *Trans. Roy. Soc. Trop. Med. & Hyg.* **29**, 113-118.

1935. (With K. S. Krikorian and R. Reitler.) The occurrence of typhoid bacilli containing Vi antigen in cases of typhoid fever and of Vi antibody in their sera. *J. Hyg.* **35**, 421-427.

1935. (With R. M. Pitt.) Virulence and immunogenic activities of *B. typhosus* in relation to its antigenic constituents. *J. Hyg.* **35**, 428-436.

1935. (With S. S. Bhatnager.) Further observations on the properties of the Vi antigen of *B. typhosus* and its corresponding antibody. *Brit. J. Exp. Path.* **16**, 422-434.

1936. (With R. M. Pitt.) The Vi antigens of various *Salmonella* types. *Brit. J. Exp. Path.* **17**, 81-86.

1936. Serum treatment of typhoid fever. *Brit. med. J.* **i**, 380-382.
1937. (With A. D. Gardner.) The serum diagnosis of enteric fever. Report and recommendations. *Bull. Hlth. Org. League of Nations*, **6**, 223-235.
1937. The serology of the typhus group of diseases and the classification of the corresponding rickettsias. *Second Intern. Congr. for Microbiol., London, 1936, Rep. of Proc.* p. 321.
1937. The significance of different bacterial antigens in infection and immunity. *Second Intern. Congr. for Microbiol. London, 1936, Rep. of Proc.* p. 359.
1937. The serum treatment of typhoid fever. *Second Intern. Congr. for Microbiol., London, 1936, Rep. of Proc.* pp. 537-538.
1938. Die Serumtherapie des Abdominaltyphus. *Schweiz. Z. allg. Path.* **1**, 50-58.
1938. (With J. Fenton and C. P. Hay.) Prophylactic use of anti-typhoid serum in a localized outbreak. *Brit. med. J.* **i**, 1090-1092.
1938. Detection of chronic typhoid carriers by agglutination tests. *Lancet*, **ii**, 738.
1938 (With G. F. Petrie.) The preparation of anti-typhoid serum in the horse for therapeutic use in man. *J. Hyg.* **38**, 673-682.
1938. The titration of therapeutic anti-typhoid serum. *J. Hyg.* **38**, 750-772.
1939. Test for Vi antibody. *Brit. med. J.* **ii**, 1253.
1940. The pathogenic and immunogenic activities of *Bact. typhosum* in relation to its antigenic constituents. *Third Intern. Congr. for Microbiol., New York, 1939, Rep. of Proc.*, pp. 798-799.
1941. A new type of typhoid and paratyphoid vaccine. *Brit. med. J.* **i**, 391-395.
1941. (With S. G. Rainsford and E. J. Stokes.) Antibody response and systemic reactions after inoculation of a new type of T.A.B.C. vaccine. *Brit. med. J.* **i**, 435-440.
1942. The typhus group of fevers—Classification, laboratory diagnosis, prophylactic inoculation, and specific serum treatment. *Brit. med. J.* **ii**, 597-601.
1942. Complement fixation in typhus fever group. *Lancet*, **ii**, 586-587.
1943. Experiences with typing of typhoid bacilli by means of Vi bacteriophage. *Brit. med. J.* **i**, 435-441.
1943. (With B. R. Callow.) Typing of paratyphoid B bacilli by means of Vi bacteriophage. *Brit. med. J.* **ii**, 127-130.
1944. Technique and interpretation of the Weil-Felix test in typhus fever. *Trans. Roy. Soc. Trop. Med. & Hyg.* **37**, 321-341.
1944. Modern laboratory methods in the control of typhoid and paratyphoid-B fever. *Brit. med. Bull.* **2**, 269-271.
1947. (With J. Craigie.) Typing of typhoid bacilli with Vi bacteriophage. Suggestions for its standardization. *Lancet*, **ii**, 823-827.
1948. Modern laboratory methods in the control of the enteric fevers. *Proc. Fourth Intern. Congr. on Trop. Medicine and Malaria, Washington*, pp. 283-289.
1949. The nature of bacteriophages. *Fourth Intern. Congr. for Microbiol., Copenhagen, 1947, Rep. of Proc.* pp. 355-356.
1949. Bacteriophage treatment of chronic typhoid carriers. *Fourth Intern. Congr. for Microbiol., Copenhagen, 1947. Rep. of Proc.* p. 363.
1950. Note on a case of Brill's disease in London. *Lancet*, **ii**, 99-100.
1950. Standardization of serological tests for the diagnosis of the typhus group of fevers. *Bull. World Hlth. Org.* **2**, 637-642.
1950. Standardization of diagnostic agglutination tests—typhoid and paratyphoid A and B fevers. *Bull. World Hlth. Org.* **2**, 643-649.
1951. Laboratory control of the enteric fevers. *Brit. med. Bull.* **7**, 153-162.
1951. (With E. S. Anderson.) Bacteriophages carried by the Vi-phage types of *Salmonella typhi. Nature, Lond.* **167**, 603.
1951. (With B. R. Callow.) Paratyphoid-B Vi-phage typing. *Lancet*, **ii**, 10-14.
1951. (With R. M. Pitt.) The pathogenic and immunogenic activities of *Salmonella typhi* in relation to its antigenic constituents. *J. Hyg.* **49**, 92-110.

1951. The preparation, testing and standardization of typhoid vaccine. *J. Hyg.* **49**, 268-287.

1951. (With E. S. Anderson.) The immunizing potency of alcohol-killed and alcohol-preserved typhoid vaccine after storage for ten years. *J. Hyg.* **49**, 288-298.

1951. (With E. S. Anderson.) Bacteriophage, virulence and agglutination tests with a strain of *Salmonella typhi* of low virulence. *J. Hyg.* **49**, 349-364.

1952. Bacteriophage not a virus? In: *The nature of virus multiplication*, Symposium, Oxford, April 1952, pp. 203-209.

1952. (With E. S. Anderson.) Variation in Vi-phage II of *Salmonella typhi. Nature, Lond.* **170**, 492-494.

1952. The properties of different *Salmonella* Vi antigens. *J. Hyg.* **50**, 515-539.

1952. The Vi antigen of *Salmonella paratyphi* A. *J. Hyg.* **50**, 540-549.

1952. The Vi antigen of *Salmonella paratyphi* B. *J. Hyg.* **50**, 550-557.

1952. The serological classification of bacteriaceae. *J. Hyg.* **50**, 558-579.

1953. (With E. S. Anderson.) 'Degraded Vi strains' and variation in Vi-phage II of *Salmonella typhi. J. Gen. Microbiol.* **8**, 408-420.

1953. (With E. S. Anderson.) The Vi-type-determining phages carried by *Salmonella typhi. J. Gen. Microbiol.* **9**, 65-88.

1953. (With E. S. Anderson.) Vi-phage-type specificity and degradation in *Salmonella typhi. Proc. Sixth Intern. Congr. for Microbiol., Rome*, **3**, 462-464.

1953. Standardization of typhoid vaccine. *Proc. Sixth Intern. Congr. for Microbiol., Rome*, **2**, 359-362.

1953. Second report on the working of the International Scheme for Enteric Phage Typing. *Proc. Sixth Intern. Congr. for Microbiol., Rome*, Appendix p. 94-97.

1954. The provisional International Standard Antityphoid Serum. *Bull. World Hlth. Org.* **10**, 911-917.

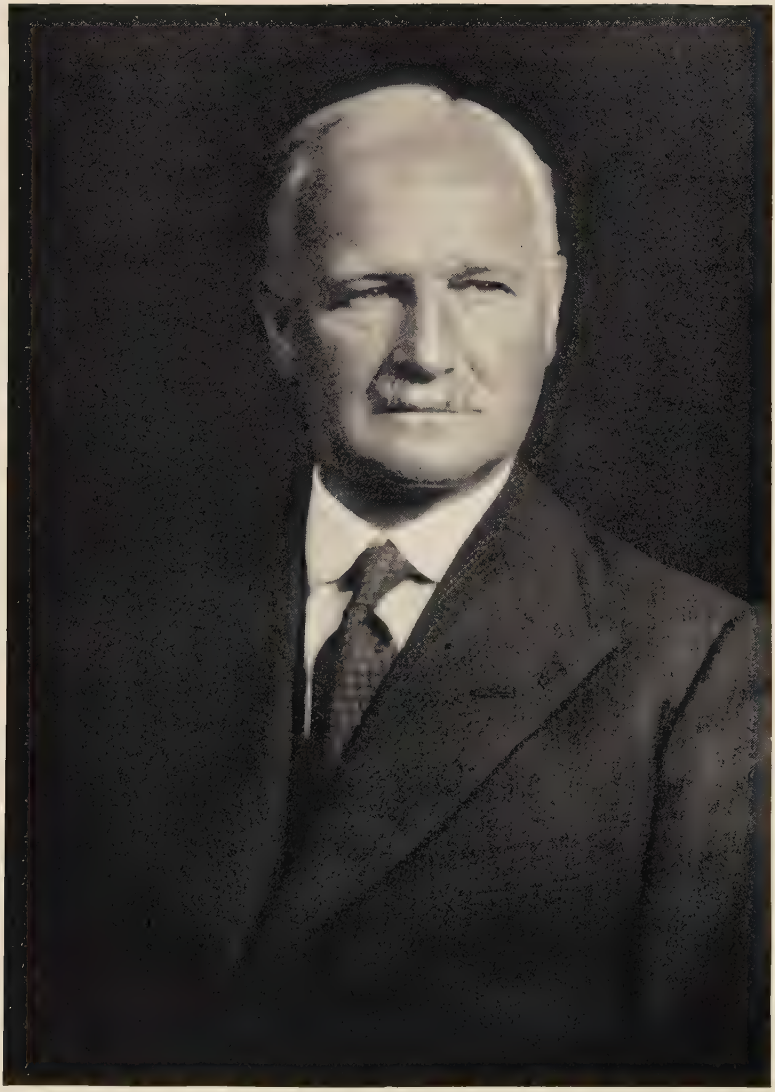
1954. (With H. J. Bensted.) Proposed Standard Agglutinating Sera for typhoid and paratyphoid A and B fevers. *Bull. World Hlth. Org.* **10**, 919-926.

1955. Discussion on 'The classification of viruses'. *J. Gen. Microbiol.* **12**, 365-366.

1955. Discussion on 'Bacteriophage and bacterial classification'. *J. Gen. Microbiol.* **12**, 380-381.

1955. World survey on typhoid and paratyphoid-B phage types. *Bull. World Hlth. Org.* **13**, 109-170.

1956. Phage typing of *Salmonella typhimurium:* its place in epidemiological and epizootiological investigations. *J. Gen. Microbiol.* **14**, 208-222.



Percival : Hartley :

PERCIVAL HARTLEY

1881-1957

Parentage and education

Percival Hartley was born on 28 May 1881 at Calverley, near Leeds, as second son and third of the four children of William Thompson Hartley and his wife, whose maiden name had been Grimshaw. The father, W. J. Hartley, was in business as a manufacturer of woollen cloth. From notes which his son Percival has left, it may be gathered that he was a failure in this business of his own, but, by way of contrast, very successful in his activities on behalf of his fellow townsmen, as a social worker, a magistrate and a member of the local Council, of which he became the Chairman.

Percival Hartley's mother also came of people connected with the local industry, an ancestor of hers having built and owned one of the first mills to make woollen cloth; and Hartley evidently thought of her as having had a stronger and more enterprising influence than that of his father, on the prospects of their family. 'She managed our schooling', he writes, 'and made the decisions which mattered, about schools, colleges etc. She encouraged us, and me particularly, to "Strike out", try new ventures, etc. She encouraged change and trying things, while my father always "played for safety".' Later he appears to have recognized more clearly that his father also had plans and ambitions for the family, but that these were largely determined by the admiration and respect which he had formed for J. R. Raper, also a manufacturer of woollen cloth, but a more successful one. He was the father of H. S. Raper, who was to become a Fellow of the Royal Society, and whose obituary memoir, when he died in 1953, was to be written by Percival Hartley (*Obit. Not. Roy. Soc.* **8**, 567). The two were nearly of the same age, Raper being by some months the younger; they were to follow one another closely, at school, technical college and university. Raper, who apparently matured rather earlier, was ahead of Hartley at each stage by a year or so, and they were to form a close and lifelong friendship. We shall see that they became members of a remarkable succession of biochemists, all of whom were pupils of the late Professor J. B. Cohen, F.R.S., at Leeds. Hartley believed that the fidelity with which his own educational course followed that of his friend Raper was largely the result of the admiration which his own father felt for Raper's—the feeling that whatever decisions J. R. Raper made for *his* son's education must be the best for that of his own son, P. Hartley;

though it will be seen, in due course, that they were to follow one another even beyond the stage of university entrance and parental influence.

Hartley went first to a village school and then, at the age of 11 or 12 to Belle Vue School, which appears to have been a high school of good standing, founded independently but, like other such, eventually acquired by the public authority, the Bradford Education Authority. This was the first school at which he encountered H. S. Raper, a year ahead of him, and they both went from it, at the same interval, to the Bradford Technical College, where Hartley held a West Riding County Council Scholarship, and then to what was still the Yorkshire College of the Victoria University when Hartley entered it in 1900, but was to become the University of Leeds. He had already felt a special attraction to chemistry, and chose it as his principal subject of study at Leeds, where Arthur Smithells, F.R.S., was then the Professor and Head of the Department of Chemistry. He remembered with appreciation the first course of lectures which he heard from Smithells; but, like Raper before him, and H. D. Dakin earlier, he soon fell under the potent spell cast by J. B. Cohen, also to become F.R.S., who was the Professor of Organic Chemistry. Cohen was himself a distinguished worker in the main current of the organic chemistry of those days; but he seems to have been ahead of most of his academic contemporaries, in foreseeing a coming re-orientation of his subject, in a direction more nearly that of its original objective—the study of the chemical products and processes of living organisms. He was a man of great personal charm and friendly disposition, and, with his special gift as a teacher, he gave his pupils the opportunity of acquiring a thorough grasp of the theoretical foundations of organic chemistry, and precision, therewith, in the practice of its methods; but he led the interests of those who were attracted to research, towards what he foresaw as the important development of its future, in which it would make new contacts with those wings of the biological sciences in which chemical methods were being used, and for which a more independent status was soon to be claimed, as 'Biochemistry'. So from Cohen's Department of Organic Chemistry there came in succession, to make their variously distinguished contributions to the emergence and development of the new Biochemistry, H. D. Dakin, H. S. Raper and Percival Hartley; and, after Hartley, H. W. Dudley who, after working for some years with Dakin, was to join me with Hartley at the National Institute for Medical Research; and H. Raistrick, A. Wormall and F. C. Happold, who were to hold Chairs of Biochemistry in London and Leeds.

Hartley graduated in the First Class at Leeds, and spent a short further period there, learning the methods of research with Cohen, with whom he published two short papers on methods of organic synthesis. Then, in 1904, he obtained a small teaching appointment at a technical school in Oxford, supplemented his resources by some private tuition, and, being left with time on his hands, used it for further research in the University Laboratory of Organic Chemistry, which apparently led, however, to no recorded results. There was some talk of his entering an Oxford College, as a post-graduate student; but at that juncture, in 1906, another opportunity was presented in London, at the Lister Institute; and this was to provide Hartley with his first research appointment, and with the opening to the second stage of his career in science.

Biochemistry at the Lister Institute

Raper had already been working for a year under J. B. Leathes at the Lister Institute, and had just returned there after a visit to Germany. Dakin had been there earlier, but had already left to join Christian Herter in New York. Hartley believed that the opportunity came to him because these two, from Cohen's Leeds 'nursery', had been there before him. However that may have been, he was offered the research scholarship provided by the Grocers' Company at the Lister Institute, in succession to R. H. A. Plimmer, and accepted eagerly this opportunity—'the greatest, I think, which ever came to me', as he wrote many years later. He welcomed, of course, the reunion which it brought with his friend Raper, and they arranged to share a lodging in Battersea. When he looked back later at the years which he spent there, Hartley wrote of the Lister Institute as one of the few centres in this country in which, at the time of his entry, work in biochemistry was being encouraged, or even recognized. It cannot be doubted, I think, that he was right. There was already a beginning in Cambridge, of course, under Gowland Hopkins, of what was to become a famous centre there; and the first Chair of Biochemistry had been founded in Liverpool, and was occupied by Benjamin Moore. Elsewhere, however, the subject, as a separate discipline, had hardly emerged. At the Lister Institute, Harden and Young had already opened their important new chapter in the chemistry of enzyme action; while C. J. Martin, who had returned a few years earlier from Australia to be the Director of the Institute, showed a definite biochemical bias in his approach to the wide range of research projects in which he was interested, and which he was ready to promote. Hartley's researches there were begun under the stimulus and experienced guidance of Leathes, with whom he co-operated in the discovery of a fatty acid with twenty-four carbon atoms and eight double bonds, which Lewkowitch later named 'arachidonic acid', and the presence of which as a natural cell-constituent has since been repeatedly confirmed. Its octohydroxy- and octobromo-derivatives were prepared and analyzed by Hartley, who also produced further evidence for the presence in the fat of the liver, and of other organs, of a much higher proportion of unsaturated fats than is present in the fat of the adipose depôts; thus giving further support to Leathes's suggestion that this desaturation of the fats in the actively metabolic cells is preparatory to the fragmentation of the long fatty-acid chains, on the way to their ultimate oxidative destruction. Hartley also found evidence for the presence in liver fat of an oleic acid with its unsaturated bond at an abnormal, asymmetrical position in the chain; but this does not appear to have been confirmed by later workers.

Hartley undoubtedly enjoyed this, his first opportunity for testing his potentialities as a worker in biochemical research. In later years, in such moods of discouragement as are not unknown to others of us who work in science, his thoughts would sometimes return nostalgically to that early experience, and he would even ask to be relieved of the major undertaking, through which he was then doing by far the most important service of his whole career to both international and national scientific interests, so that he might begin again to concentrate the whole of his efforts and attention on a single set of problems, such as that of the metabolism of the fats. At other times his more serious judgment would certainly have recognized that, while his early work on the fats had been sound, and had shown his capacity for thorough and accurate work, it had not given promise of conspicuous originality, or of such distinction as he was eventually to win in a much wider field, where he would find scope for administrative and personal qualities, as well as for great scientific competence.

Veterinary immunology at Muktesar, India

The scholarship with which Hartley had entered the Lister Institute came to an end in 1909, by which time he had obtained the degree of D.Sc. at the University of London. His next move was to be to India, where he had been appointed Physiological Chemist to the Government of India. He learned that he was to be posted to what was then known as the Imperial Bacteriological Laboratory, at Muktesar, a hill station in the United Provinces, where he would be called upon to do research on infections which were creating serious problems for the breeding and maintenance of horses and cattle in India, and on the production of immunological remedies for some of them. He accordingly spent some months, before he sailed for India, at the Lister Institute's serum establishment at Elstree, obtaining his first experience of practical immunology with Drs MacConkey and Sidney Rowland. At Muktesar he worked for a time on the concentration of arsenic in the blood serum of animals, after the administration of different arsenical compounds by different routes, in connexion with the rather primitive chemotherapy then on trial, of such diseases as 'surra'—a disease of camels and horses due to a trypanosome infection. While he was in India Hartley met Griffith Evans, the distinguished veterinary officer who had discovered the '*Trypanosoma evansi*', the cause of this disease. Hartley's chief interest, however, and his most important research activities at Muktesar, were concerned with the study of the changes in the serum proteins of animals suffering from, recovered from, or immunized against the serious cattle plague—Rinderpest. His duties included the practical testing of curative sera, prepared at Muktesar for the treatment of Rinderpest and anthrax; and he was impressed with the advantages of being able, for such veterinary purposes, to prepare and test the sera in the same animal species, as that in which they were to be used for treatment. He thought that this experience had sown the seed of ideas which were to germinate much later, when, in his closing years, he was gathering evidence for the superiority of an homologous antitoxin over one prepared in an alien species, even in human therapeutics, if it could be made available. In any case, there can be no doubt that the four years which Hartley spent in India gave him further experience of practical immunology, under conditions calling for personal initiative and contrivance, which made an important addition to his equipment for what was eventually to be the main scientific work of his maturity.

Return to the Lister Institute. Outbreak of War, 1914

Hartley returned in 1913 to the Lister Institute, where, as the result of a reorganization then in progress, a post was available to him as first assistant to Arthur Harden, the head of the Department of Biochemistry. His duties involved a general responsibility for the needs of a number of research workers who were then attached to the Department; and, as his own research project, he prepared, in a state of as great purity as the methods then available made possible, the proteins then recognized as chemically separable—euglobulin, pseudoglobulin and albumin—from the blood serum of both the horse and the ox, and compared them with respect to their respective contents of free amino-nitrogen groups. He was apparently coming to the end of his investigations on this line, when the outbreak of the first World War, 4 August 1914, fell like a bomb, and almost without warning, on the plans and programmes of those of us who were engaged in scientific researches of almost any kind, and especially of those who were still of military age. And it was then that I made my own first closer contact with Hartley, and at the Lister Institute.

I myself had then only recently taken up my new appointment with the Medical Research Committee (now Council). The building which had been acquired to accommodate the projected National Institute for Medical Research had, naturally, been promptly offered for use as a military hospital; and I had been sent with my colleagues to temporary accommodation in the Lister Institute, with a firm injunction to stand by for the work of national importance which would soon require our research services, as was to happen in due course. It was thus that I encountered Hartley, who, my junior by some six years, had naturally made prompt application for a commission in the army, which he was not to be granted till a year later, but then under conditions which offered much better scope for his special scientific equipment, than those which fell to the lot of many others. Meanwhile there were various services, in connexion with the Lister Institute and beyond it, on which he could be employed in the interval of waiting; such as the concentration of a stock of tetanus antitoxic serum at Elstree, the improvement of the medium for preparing typhoid vaccines, and the inauguration of a department of chemical pathology at the King George the Fifth Military Hospital. Even so he still had time to spend at the Lister Institute, and I found him keen to interest me, while I waited for more problems of national urgency, in the status as anaphylactic antigens of the pure serum proteins which he had prepared. So we worked together at odd times in that year, using the method for detecting the anaphylactic condition in an isolated strip of plain muscle from a guinea-pig, which I had developed some years earlier, and which, by giving an objective record of the response, made possible a finer discrimination. The subject had, indeed, been in some confusion. The experimental results already available had appeared to indicate that the essential anaphylactic antigen in a foreign serum, producing the characteristic abnormal sensitiveness in a guinea-pig, was the euglobulin alone; the pseudoglobulin having also some sensitizing effect, but presumably as the result only of imperfect separation, while the albumin was said to have none at all. Using the test *in vitro*, on the isolated plain muscle, Hartley and I were able to show that the euglobulin and pseudoglobulin, both from horse serum, were, in fact, distinct anaphylactic antigens, though naturally with some overlap in their specificities; while the more easily purified albumin was also a separate and a perfectly good antigen, the sensitizing action of which had only been missed on account of the much longer interval required for the development and fixation of its antibody, and the consequently much later appearance of the specific sensitiveness to albumin. Our work together on a problem of this fundamental kind had come as a welcome, even as a useful relief, from war-time worries and demands; and I think that it had a further value for both of us, in view of our later association, in giving us an assurance that we could work together easily.

Sanitary Officer in France

Hartley received his commission in August 1915, as a Lieutenant in the City of London Sanitary Company of the Royal Army Medical Corps, in which a number of other members of the Lister Institute staff were his fellow officers. He was sent to France in the autumn of that year as the Officer Commanding the 81st Sanitary Section, attached to 16th (Irish) Division, which he had joined at Southampton. His duties were concerned with various matters of field sanitation; and researches which he recalled in that connexion dealt with such practical problems as the supply of water to troops in battle, the disposal of the carcases of dead animals, the manufacture of soda-water from chlorinated water, and the demonstration of the ineffectiveness of the methods which had been introduced for freeing troops from infestation with lice, by exposing the body to air containing chlorine, or sulphur dioxide. He demonstrated the continued viability of the eggs of lice after such treatment, by wearing them in gauze-lidded boxes attached to his skin—a method devised by A. E. Bacot at the Lister Institute. This appears to have been regarded as an act of extraordinary heroism by military colleagues accustomed to facing only bullets and shells with equanimity. It must certainly, however, have been for conspicuous gallantry in a more serious direction that Hartley, who had become a Captain, R.A.M.C., in the spring of 1916, was awarded the Military Cross in 1917. His Section had served with its Division in a series of famous battle areas, and it was during the battle of Passchendaele that he at length received orders to leave it, with an enthusiastic citation from the General Commanding the 16th Division, and to proceed to Rouen to join C. J. Martin, who was then serving as Adviser in Pathology with the rank of Lieutenant-Colonel. In this intimate association with Martin in research and routine, with almost daily calls and opportunities for the improvisation of technical expedients, Hartley obtained further and invaluable experience in practical bacteriology.

At the Wellcome Physiological Research Laboratories

When Hartley was demobilized, in 1919, there was apparently no suitable appointment available for him at the Lister Institute, and I remember his coming to me for advice, before committing himself elsewhere. I had no opening suitable for him then; but I certainly had at the back of my mind the probability, which I was beginning to foresee, that the Medical Research Council would eventually find it necessary to create in the National Institute for Medical Research, when it had been established in the building earlier acquired for its reception, a special department for the preparation and preservation of official standards for remedies, such as the antitoxins among others, the potency and safety of which could only be controlled by comparative biological assays and tests. Hartley had many obvious qualifications for the responsible work of such a department; but there was no certainty yet that it would ever come into existence, while the date at which it might do so was still completely vague. When Hartley, however, told me that he had been offered an appointment in the Wellcome Physiological Research Laboratories, of which I myself had been the Director until shortly before the War, it seemed to me that he ought, in any case, to find an opportunity there which would be entirely congenial to his special interests and qualifications, and which ought, at the same time, to strengthen his equipment for dealing with these biological standards, if a national centre to be responsible for these should eventually be formed, and if he should then wish to be concerned with it. I advised him, therefore, to consider the offer from the Wellcome Laboratories, and was glad to learn that he had accepted it. During the two years which he spent there, he certainly did work of first-rate importance to practical immunology, and especially to the work of the numerous centres, in many countries, where the diphtheria antitoxin was being produced. Everybody engaged in that production had been all too familiar with the apparently capricious and uncontrollable variations in the yield of good toxin by cultures of the diphtheria bacillus, even of the strains which, at their best, were known to be excellent toxin producers. Nobody could foresee, apparently, when his supply of useable toxin might suddenly fail. Hartley was able to show that these vagaries were, in fact, not due to an inherent instability in the behaviour of the organism, but to unappreciated variations in the composition of the broth used for its culture. He was then able to lay down precise conditions for the preparation of an effective broth and tests for their fulfilment. The introduction of what came to be known as 'Hartley's broth' thus removed what had been a major impediment to the regular production of potent diphtheria toxin, and therewith of antitoxic sera of high value. Later Hartley devised a form of the prophylactic antigen, for use in the production of active immunity against diphtheria in the human subject. It consisted of a suspension of the specific precipitate ('floccules') formed by the modified diphtheria toxin when mixed, in accurately neutralizing dilutions, with the antitoxin; and this had a definite, though eventually a somewhat limited, practical use. In all this work Hartley was most fortunate in having the co-operation of Miss Olga Parnell, a science graduate from Cambridge, who had entered the Wellcome Laboratories to do work of national importance during the War, and with whom, in due course, he made an ideally happy marriage. An infected hand, however, interrupted Hartley's scientific activities in 1921; and what may well have been a late reaction from the strain which his service in the War must have imposed, and which he had seemed to bear easily while it lasted, made him then resign his appointment at the Wellcome Laboratories and retire into the country with his wife for a good part of a year. When he had recovered, the fact that the projected control centre for Biological Standards at the National Institute would soon be needed, was already becoming clear. Hartley was accordingly appointed by the Medical Research Council to the Staff of the Institute in 1922, to take what steps might be required to prepare for a suitable organization. For this he was to be initially responsible to me; but it soon became clear that the importance of the undertaking would involve the constitution of a department of a special kind, with Hartley as its director.

National Institute for Medical Research. Department of Biological Standards

Hartley thus entered upon an appointment which he was to hold for the next 24 years, till he reached the official age for retirement, in 1946. This was by far the longest section of his scientific career, during which his activities, in research or administration, were mainly and continuously occupied with one set of problems; and I am confident that, in retrospect, he would have agreed that it enabled him to render by far the most important of all his services to medical science and its practical applications. The reference to him as Director of a Department should not be misunderstood, however, as indicating that he had a large staff at his disposal, with each member of it accepting work on one or more of the standards as his principal duty. Such a plan, if it had been adopted, would have become increasingly difficult and disproportionate, in relation to both the accommodation and the financial provision available to the Institute as a whole, as the number and the variety of the required standards rapidly increased, and, with these, the number of the special kinds of knowledge and technical experience needed for their expert application. Hartley himself, with the one, or at most two, qualified colleagues who, at one time, were primarily engaged with him on researches concerning the immunological standards, had only two rooms at disposal, for their experimental work and also for the correspondence and documentation involved by the duties of the Department as a whole. The researches and experimental work involved by the standards for remedies of other kinds—insulin, pituitary posterior lobe, salvarsan and its derivatives, digitalis and its analogues, vitamins, hormones of the gonads, adrenal cortex and anterior pituitary lobe—were farmed out, as it were, among the various experts working in other departments of the Institute, who accepted such duties as additional to their main research commitments. Such an arrangement was obviously economical in expert potential; and, even from the point of view of some, at least, of the people concerned, there was something to be said in favour of providing such a secondary duty, of a more formal and definite nature, to which a whole-time research worker could turn, with a sense of continuing to justify his existence and his salary, if his own special research, as so often happens, should appear to have reached a temporary impasse. On the other hand, it could not be doubted that special qualities of tact and friendly understanding would be required of the Director of a Department thus loosely organized, to enable him to get the work on the standards done with the requisite promptitude and precision, without encountering personal difficulties with the specialists concerned, or with the Directors of the special Departments to which they were primarily attached. It says much for the attractive character of Hartley's personality, for his spontaneous good feeling and for the respect aroused by his enthusiasm and his perfectionist idealism, that an enterprise organized on such a basis should have achieved so much, and with so little trouble.

Hartley's new appointment had been made none too early. Dr Thorvald Madsen, Director of the State Serum Institute, Copenhagen, who had then recently become the President of the Health Committee of the League of Nations, had arranged for the meeting in London in the previous year, 1921, of an international conference to discuss the provision in some internationally recognized centre, from which samples could be distributed, of the standards of reference for the antitoxic sera, for which nearly all countries had been dependent, before August 1914, on Ehrlich's Institute for Experimental Therapeutics in Frankfurt a/M., from which supplies had inevitably been interrupted during the years of war. This London Conference had recommended the recognition of the Copenhagen Institute as such an international centre, and had provisionally decided to adopt Ehrlich's unit of diphtheria antitoxin, as based on his Frankfurt Standard, as the basis for the international standard and unit now to be provided. International action having thus been initiated to deal with the serum standards, Madsen consulted me, early in 1923, about the possibility of similar action with regard to potent remedies of other kinds, the activity of which also could be only determined, and indicated, in units measured by biological tests; and I had replied that there were cases, in particular those of insulin, then recently becoming available, and of the pituitary posterior-lobe extract, in which it appeared to me that such action was even more urgently needed than in that of the antitoxins, since no stable reference-standards had yet been provided from any source, in relation to which the activity of such dangerously potent remedies could be accurately indicated in generally accepted units. Two successive, internationally representative Conferences were accordingly arranged, in Edinburgh in 1923 and in Geneva in 1925, at the second of which stable standards, with internationally valid units defined in terms of them, were accepted for insulin and the post-pituitary extract; and the new British Department of Biological Standards, of which Hartley was now the Director, was requested to accept the responsibility for preparing and preserving these, and for distributing samples as required to other national centres. Part of the same international standard preparation of insulin was to be kept in Toronto, by the Insulin Committee there, for supply to producers in the New World. Similar action was foreseen in the case of remedies of the digitalis group, and of salvarsan and its analogues, and the British centre was asked to take appropriate measures with regard to these also, so as to prepare for the adoption of international standards for them at a future conference.

Meanwhile things had moved in this country. At the prompting of the Medical Research Council, the Ministry of Health appointed a Departmental Commission to report, in 1923, on the measures required for obtaining effective official control over those therapeutic substances of which the potency and purity could only be assured by biological assay and testing; and, as a consequence, a 'Therapeutic Substances Act' was passed in 1925, and was due to take effect in 1927. Before that time, therefore, a national centre would be required, in which the standards of reference for the substances scheduled under this Act would be prepared, preserved and made available, to meet the need for them in Britain, and from which expert advice could be given to whatever organization in the Ministry of Health might be charged with the responsibility of ensuring that these Standards were effectively applied, by the holders of licences under the Act. Hartley's Department thus became responsible for having ready, well before 1927, the official British Standards for the whole range of the 'therapeutic' substances which were to be included in the original schedules to the Act, and, in due course, for any others which might later be added with the approval of Parliament, as experience might show the need for such additions, and as new remedies might from time to time appear—they still do so in fact—for which control under the Act would be required, if their safe and effective use was to be ensured.

Hartley accordingly found himself presently responsible for the preparation and the custody, under conditions ensuring their permanent stability, of all the official standards for the substances scheduled under the British

Therapeutic Substances Act, and for the distribution of samples of these, as required, to the holders of licences for the manufacture and sale in Britain of the corresponding products. Of these standards some 12, as shown in a list which he gave me for information in 1939, were immunological standards, the weight of the British standard representing one international unit being determined, in each case, with the greatest possible precision, by comparison with the corresponding international standard kept in Copenhagen. When Denmark was occupied by the German army during the second World War, it was, therefore, possible for Hartley's Department in Hampstead to take over, for the time being, the distribution of these exactly equivalent British serum standards on the international account, to such other and still accessible national centres as required them, as well as to the British licensees. For all the other standard preparations, 19 of them already in 1939, for the digitalis drugs, the arsphenamine (salvarsan) derivatives, the vitamins and the still growing series of the hormones, the Hampstead Department had, from the first, the responsibility for the preparation, custody and distribution of these on the international account, as well as to meet the British legal requirements; so that the international and British standards, for all these non-immunological remedies, were of exactly the same material in each case. Later Hartley's Department accepted a further responsibility for keeping and supplying all these standards, as the different remedies which they represented, and the requirements for the biological assay of these, were introduced into new editions of the *British Pharmacopœia*. He personally served on the Commission of experts responsible, under the authority of the General Medical Council, for the production of successive *Pharmacopœias*, and took, in that capacity, the main responsibility for the drafting of those Monographs and Appendices which dealt with the substances in question, with the official standards for them, and with the acceptable tests for biological assays in comparison with these standards.

Further researches

The account which I have so far given of Hartley's main activities for nearly a quarter of a century, during which he worked at the National Institute for Medical Research, must not be allowed to leave an impression that his time and attention had been so completely absorbed by his responsibility for an administrative routine, as to prevent him from still making contributions of importance to the more general advancement of scientific knowledge, during this major section of his working career. Reference to the bibliography of his scientific publications, which is attached to this notice, will show, indeed, that most of his publications during this period, dating from 1923 to 1946, were concerned with investigations which had been required, or suggested to him, by problems and projects connected with his work on the Biological Standards, both national and international. Most of these researches, nevertheless, were of such a character as to give them high standing among the contemporary investigations in the general field of immunology, whether they dealt with questions of a more fundamental interest and significance, or with more practical problems, such as those concerned with the determination of the optimal methods and conditions for the artificial conferment or evocation of a specific immunity, the latter involving the measurement of an immune substance in the blood of the immunized subject with the greatest quantitative accuracy, in order to examine the relation between the amount of it in circulation, and the resistance conferred by its presence against a corresponding infection, as determined by more direct evidence. And among the investigations of this latter kind, a place of special distinction may be given to that recorded in the co-operative Report, Number 272 in the series of the *Medical Research Council's Special Reports*, entitled 'A Study of diphtheria in two areas of Great Britain'. Hartley continued to work on this report for some years after his retirement from his appointment at the National Institute in 1946, and until it was passed for publication in 1950. And, though he shared the responsibility for this report with eight co-authors, he should be credited with a share of major importance, not only in the planning of the investigation as a whole, but in the carrying out, at one stage with Lady Hartley's efficient collaboration, of the quantitative serological determinations required to complete the experimental evidence; in the framing and writing of much of the text from data supplied by his eight co-authors; and, finally, in the co-ordinating and editorial functions involved in presenting all the information of so many different kinds, and from so many different sources, as a coherent report. It provided, in fact, an invaluable assemblage of information, based mostly on original evidence, about the different factors—types of antigen used, number and spacing of injections, types of the bacillus concerned in different outbreaks, etc.—which could influence the result of the protective inoculation against diphtheria in the direction of success or of failure. Some years earlier the Ministry of Health had launched a nation-wide campaign for the general acceptance of this antidiphtheria inoculation for the nation's children, and was still actively promoting it; and I think that there can be no doubt that the information, which was made available by Hartley and his collaborators in this report, was a significant factor in enabling that campaign eventually to achieve such a degree of success, that diphtheria, till recent years one of the greatest threats to child life, has now been reduced almost to insignificance in Britain, after having been eliminated altogether in a number of the major centres of population in the North American Continent, where it had been possible to apply the protective inoculation earlier than in this country, and with a more systematic completeness.

There was one other investigation to which Hartley devoted much time and interest in his period at the National Institute, and which seems to me to be worthy of special record here, partly because of what I still regard as its intrinsic scientific interest, but more particularly because, most unfortunately, he himself left no adequate published account of it. I have to trust to my memory of what happened upwards of 15 years ago; on the other hand, the subject was one which interested me so directly, that I believe that I kept more closely in touch with the progress of Hartley's observations, in this particular research, and had more regular discussion with him about the meaning of his findings, than with any other of those conducted by my colleagues at the National Institute, apart from researches in my own Department in which I was personally engaged. In this investigation Hartley was concerned with a remarkable difference between the specific precipitins, as present in the sera of immunized animals of different natural species, with respect to the power of transferring to a guineapig, by prior injection, a specific anaphylactic sensitiveness to the corresponding antigens. To mention the case which first brought it to notice, Hartley had found, fairly early in his Hampstead period, that guineapigs could not thus be made anaphylactic to a pneumococcus polysaccharide by a prior injection of a specific precipitin, which, as it happened, he had obtained from an immunized horse. Not long afterwards, and before he had published this negative finding, workers in the Rockefeller Institute* had shown that guineapigs could be so sensitized against such a polysaccharide by a prior injection of the corresponding precipitin, if this had been obtained from an immunized rabbit; on the other hand, they found, as Hartley had done, that a precipitin from a horse had not this sensitizing action. Further experience made it clear that a horse precipitin, in general, had no power of conferring upon a guineapig the 'passive anaphylaxis' to a corresponding antigen, such as a rabbit precipitin had long been known to possess. Other analogous examples began to come to notice; I remember, for example, bringing home, from a visit to Baltimore, the news that the late Professor Longcope had found that a precipitin from a rat would not thus make a guineapig sensitive to the antigen against which the rat had been immunized. Hartley thus came to recognize a new kind of difference between the protein antibodies, as found in the sera of different animal species, having nothing to do with their own antigenic specificity, and no apparent relation to their zoological affinities; and in his discussions with me we came to speak of this difference as due to different degrees of 'acceptability' to the living cells of the guineapig—the different degrees to which the precipitins from different species could be taken up by the protoplasm of the guineapig cells, and so far incorporated as to render these cells for the time specifically sensitive to the corresponding antigens, until the guineapig's own immunity reaction caused them to be eliminated. The sera from the different species which Hartley tested for this 'acceptability' to the guineapig tissues seemed to arrange themselves, on this basis, in an arbitrary and unpredictable series, those from the horse and the rat being at zero, those from the ox, man and rabbit in that order of increasing effectiveness, and that from an immunized guineapig the most 'acceptable' of all, as was perhaps to be expected. Anomalies began to be resolved. It had been supposed, for example, that a guineapig could not be made to give an immediate, anaphylactic response to diphtheria toxin, by giving it a previous injection of the antitoxin. Hartley now found that this failure had been entirely due to the use of the familiar horse antitoxin, so easy to obtain, but 'unacceptable' to the guineapig's tissues; and that with an antitoxin from a rabbit, or, still better, from a guineapig, a guineapig could be made passively anaphylactic to diphtheria toxin, or toxoid, as readily as to any other 'precipitable' antigen, and quite apart from the specific toxicity.

---

* Avery & Tillett, 1929, *J. exp. Med.* **49**, 251.

Throughout these experiments Hartley had tested the sensitization of the guineapigs by the method using the horn of a virgin uterus as an isolated strip of plain muscle, which he had earlier used with me. It made possible a more accurately quantitative discrimination between different degrees of sensitiveness, than the method using intravenous test injections into series of living animals. It seemed to me that Hartley had further improved the quantitative potentialities of this isolated-muscle method very greatly, by his discovery that the uterine horns removed from normal guineapigs could be passively sensitized, by exposing them for a day or two, in the cold, to known saline dilutions of the precipitin on trial for its 'acceptability', and then suspending them in the usual way in a warm saline bath for the testing with the antigen. He did publish a brief description of this elegant and valuable improvement in technique, in the *Proceedings of the International Congress of Microbiology*, which met in London in 1938; and he was further preparing to make a detailed publication of the results of the whole of this investigation, on which he had been engaged over a good many years, when a paper came to his notice, which he had previously overlooked; other investigators, working quite independently, had anticipated him in the publication of his findings in principle, and to a substantial extent in detail. To me it seemed that, disappointing though such a coincidence would naturally be, Hartley ought still to publish his results in full, as giving valuable confirmation at least, by different methods, of what others had already recorded, and, further, an extension of their findings in certain important directions. He could not be persuaded at the time, however, to deal further with this matter. And, indeed, there were plenty of others making urgent claims upon his attention; for the threat of a second World War was already beginning to cause a preparatory diversion of scientific activities into abnormal channels. And then my official relations with Hartley came to a natural end, some years before this war was finished, when I retired from the National Institute in 1942.

My cordial personal relations with Hartley had, of course, continued; and he often promised me that, when he had succeeded in freeing himself from other obligations, he would return to the 'acceptability' story, pick up the threads where he had dropped them, and, perhaps, extend his findings to a stage at which he might yet think it suitable to publish them all. That opportunity never came, however; and I have thought it desirable to record rather fully what I remember of the position as he left it, because of a feeling that the curious distribution of 'acceptability' to the guineapig among other animal species might well be worthy of further investigation, as having some probably interesting biochemical basis, and possibly even some deeper biological significance.

For two years after Hartley retired in due course, in 1946, from the National Institute, he held a temporary appointment at the London School of Hygiene and Tropical Medicine, and another, for a year, in the Department of Pathology of Oxford University; and during all this period he was still working at the co-operative Report on diphtheria. And then, at last, the opportunity seemed to have been provided by an invitation from the then Director of the Lister Institute, Sir Alan Drury, who offered Hartley a temporary research appointment, renewable from year to year, at that home of his apprenticeship into medical research, the object still of so much of his deep-rooting loyalty and nostalgic affection. Hartley accepted the invitation with the eagerness which was to be expected; and I hoped that he would still be able now to resume the broken investigation, with much, at least, of the zest and scientific insight of his earlier years. That hope, however, was doomed to disappointment. He seemed to me, indeed, to be trying to make a completely fresh start, and to find an application of the knowledge already in hand to the antitoxin treatment of diphtheria, on which so much of his thought had been engaged in the intervening period. And then, all too soon, his friends began to detect in his movements the first signs of a disability, which was thereafter to advance, slowly but inexorably through the following years, till his death in February 1957.

When Hartley retired from the National Institute at Hampstead, in 1946, it was natural that, in published acknowledgments, attention should have been concentrated on his outstanding services to therapeutic science and its practical applications, by his work, both national and international, for the establishment of a uniform series of Biological Standards. The *Report of the Medical Research Council* for the years 1945-1948 made more than one reference to Hartley's retirement, and the following two passages from that *Report* (pp. 36 and 47) are worthy of quotation, I think, as summarizing the more general appreciation of his achievement, by those viewing it from a less intimate angle than my own: 'Carrying as he did a considerable burden of responsibility in connection with the work on standards, Sir Percival nevertheless maintained a continuous output of original researches on immunological subjects; in losing him the Institute has lost one of the most distinguished men who have served on the Council's staff' (p. 47). 'His experience and equipment, combined with exceptional personal qualities, fitted him in a high degree for the special work he undertook at the National Institute. In this work he was responsible not only for the actual development and preparation of biological standards of many substances in the field of hormones, vitamins and other natural products, but also for the organization of the national and international investigations which were the essential preliminary to the adoption of all such standards. In addition, he formed the main link between the Council and the Ministry of Health in the administration of the Therapeutic Substances Act. In this way he showed himself to be not only a first-class scientific worker, but also an excellent administrator with the outstanding faculty of working harmoniously with scientists of other countries' (p. 36).

Personal memories and conclusion

Before he was overtaken by his final affliction, Hartley had suffered another personal tragedy, in the early death of his wife in 1950. Since they met as workers in the Wellcome Laboratories and were married, Lady Hartley had been for him a perfect companion and comrade in all his interests, scientific as well as personal and domestic; and, when the marriage of both their daughters had left her again with greater freedom, she had resumed such a direct co-operation with him, in the laboratory, as had been the occasion of their first meeting. Then the stress of middle life and the accident of a fractured ankle had sent her into hospital for a spell; but she was ready to be discharged, and he had gone joyfully to bring her home, only to be met with the news of her sudden death, from a pulmonary embolism. It was a shattering blow, but he had taken it with admirable courage, and had settled again into a quiet period, happy in his work and cheered by the visits of his daughters and their children. And then came the further tragedy of the insidiously disabling illness, which progressively deprived him of muscular power, first for locomotion and, in its later stages, even for intelligible speech; though, till the very end, his mind and his sensory appreciation had apparently remained but little impaired, and he was eager still to greet the friends who visited him, and who were able still to recognize the warmth of his affectionate interest in their affairs.

Percival Hartley, indeed, had been one of the friendliest and most spontaneously co-operative of scientific colleagues, in each of the research centres where he had found his scientific opportunities. At the National Institute at Hampstead, where he spent most of his working years, for some twenty of which I had the happiness of knowing him as a colleague, we soon came to recognize Hartley as the natural initiator and organizer of means for the encouragement of personal contacts and community of scientific interests, among the members of the different research departments, with their rather widely various ranges of problems. He was, for example, the moving spirit in the organization of periodical staff colloquia, to keep the staff as a whole in intelligent and critical touch with the researches in progress in any of the departments. I myself remember, with lasting personal gratitude, an occasion during the period in which London was under daily and nightly threat from the air, when a special scientific recognition which had come to me, and through me, as then its Director, to the Institute, was celebrated by such a simple festivity as the emergency conditions allowed; and it had seemed natural to everybody that this should be organized by Hartley, and that he should express the congratulatory sentiments of all his colleagues. He was, indeed, a very staunch friend and loyal colleague to all of us; and none of us, I think, could have failed to be to some extent affected by the implacable, the almost fanatical perfectionism which he imposed upon himself, and demanded of all who took part with him in the standards work.

Hartley, whose service in France had won him the M.C., was awarded the C.B.E. in 1936, and knighted in 1944. He was a member of the Pathological Society of Great Britain and Ireland, of the Physiological Society, the Biochemical Society and the Society of General Microbiology. He was elected to the Fellowship of the Royal Society in 1937, serving on the Council and on the Sectional Committee for Physiology and Medical Sciences in 1943, and on the Government Grants Board H, as its Chairman, in 1946-1948. He gave the William Withering Lectures (Birmingham) in 1936, the Robert Boyle Memorial Lecture (Oxford Jun. Sci. Soc.) in 1944, and the Dixon Memorial Lecture (Roy. Soc. Med.) in 1944, as well as other lectures by invitation to the Manchester Medical Society, to the Medical Schools of Bristol, Leeds and Manchester, and to the Osler Club; and his subject, in all these cases, was concerned with the Biological Standards, or with the conquest of diphtheria by preventive inoculation.

It must be clear, I think, that a large part of the detailed information required for this Memoir would have been difficult, or even impossible, to obtain, if Sir Percival Hartley had not deposited with the Royal Society a record of the salient points in his life history and his scientific career.

H. H. DALE

BIBLIOGRAPHY

(Articles written by the author when he was Director of Biological Standards, dealing with replacement of international standards, data leading to adoption of these, and their practical use, etc., not included.)

1904. (With J. B. COHEN.) The nitration products of the isomeric dichlorobenzenes. *J. Chem. Soc.* **85**, 865.

1905. (With J. B. COHEN.) The progressive chlorination of benzene in presence of the aluminium-mercury couple. *J. Chem. Soc.* **87**, 1360.

1907. On the nature of the fat contained in the liver, kidney and heart (I). *J. Physiol.* **36**, 17.

1908. (With A. MAVROGORDATO.) On the nature of the fat contained in normal and pathological human livers. *J. Path. Bact.* **12**, 371.

1909. On the nature of the fat contained in the liver, kidney and heart (II). *J. Physiol.* **38**, 353.

1912. The preparation of rinderpest anti-serum by means of diluted virulent fluids. *Memoirs of the Ind. Civil Veterinary Dept.* No. 3, p. 231.

1914. On the precipitation of the immune bodies occurring in anti-rinderpest serum. *Dept. of Agriculture in India, Veterinary Series,* **1**, 178.

1914. The quantitative changes occurring in the serum proteins of animals during an attack of rinderpest. *Dept. of Agriculture in India, Veterinary Series,* **1**, 205.

1914. The quantitative changes occurring in the serum proteins of animals during immunization and hyper-immunization against rinder-pest. *Dept. of Agriculture in India, Veterinary Series,* **1**, 222.

1914. The determination of the composition of the different proteins of ox and horse serum by the method of Van Slyke. *Biochem. J.* **8**, 541.

1915. The free amino nitrogen of the different proteins of ox and horse serum. *Biochem. J.* **9**, 269.

1915. Methods adopted in the estimation of nitrogenous constituents of extracts derived from albuminous substances. *Analyst.* **40**, 320.

1916. (With H. H. Dale.) Anaphylaxis to the separated proteins of horse serum. *Biochem. J.* **10**, 408.

1918. (With C. J. Martin and F. E. Williams.) Agglutination in the diagnosis of dysentery. *Brit. Med. J.* **1**, 642.

1919. On the agglutination of *B. influenzae* (*Pfeiffer*) by the serum of patients suffering from influenza. *Special Report Series*, No. 36 (Medical Research Committee), p. 108.

1920. (With C. J. Martin.) The apparent rate of disappearance of diphtheria bacilli from the throat after an attack of the disease. *Proc. Roy. Soc. Med.* (*Section of Epidemiology*), **13**, 277.

1922. (With O. M. Hartley.) The routine preparation of diphtheria toxin. *J. Path. Bact.* **25**, 458.

1922. (With O. M. Hartley.) Toxin and reaction changes produced by the diphtheria bacillus in culture. *J. Path. Bact.* **25**, 468.

1922. The value of Douglas's medium for the production of diphtheria toxin. *J. Path. Bact.* **25**, 479.

1923. (With A. J. Eagleton and C. C. Okell.) The desiccation of serum and other protein solutions, with special reference to the reagents used in the Wassermann reaction. *J. Path. Bact.* **26**, 53.

1923. On the identity of the toxins produced by serologically different strains of *bacillus diphtheriae*. *Lancet*, **1**, 17.

1924. The effect of radiation on the production of specific antibodies. *Brit. J. exp. Path.* **5**, 306.

1924. Chemical structure and specificity. *Brit. Med. J.* **2**, 1099.

1924. *The 81st Sanitary Section. History of the 1st London Sanitary Company.* Chap. VI, 59.

1925. The antigenic properties of precipitates produced by the interaction of diphtheria toxin and antitoxin (I). *Brit. J. exp. Path.* **6**, 112.

1925. Observations on the rôle of the ether-soluble constituents of serum in certain serological reactions. *Brit. J. exp. Path.* **6**, 180.

1926. The antigenic properties of precipitates produced by the interaction of diphtheria toxin and antitoxin (II). *Brit. J. exp. Path.* **7**, 55.

1928. Experiments on the purification and concentration of scarlet-fever toxin. *Brit. J. exp. Path.* **9**, 259.

1930. (With G. W. Dunkin, E. Lewis-Faning and W. T. Russell.) A comparative biometric study of albino and coloured guineapigs from the point of view of their suitability for experimental use. *J. Hyg. Camb.* **30**, 311.

1931. The effect of physical and chemical agencies on the properties of antigens and antibodies. *A system of bacteriology* (Medical Research Council), **6**, 224.

1931. State control of therapeutic substances. *Brit. Med. J.* **1**, 1072.

1931. Insulin and preservative. Note on the effect of insulin solutions of varying reaction on the viability of Staphylococci. *Lancet*, **2**, 582.

1931. An international investigation relating to the adoption of a standard for gas gangrene antitoxin (perfringens) and the definition of a unit in terms of such standard. *League of Nations III* (*Health*), p. 13.

1931. On the method of standardization of toxins used for the Schick test. *League of Nations III* (*Health*), p. 28.

1933. (With H. J. Parish, G. F. Petrie and Wilson Smith.) A provisional stable standard for antipneumococcus serum, Type I, and its practical application. *Lancet*, **2**, 91.

1934. (With S. R. Douglas.) The effect of dilute solutions of certain antiseptics on the viability of tubercle bacilli. *Tubercle,* **16**, 97.

1934. (With S. R. Douglas.) The sensitization of guineapigs following a single injection of a small dose of dead tubercle bacilli. *Tubercle,* **16**, 100.

1934. (With S. R. Douglas.) The preparation of old tuberculin by the use of synthetic media, with observations on its properties and stability. *Tubercle,* **16**, 105.

1935. The state control of diphtheria prophylactic in Great Britain. *Wissensch, Woche zu Frankfurt a/M.* **3**, 81.

1935. (With Wilson Smith.) A proposed international standard for antipneumococcus serum (type I). *Bull. Hlth. Org.* **4**, 48.

1935. (With M. Llewellyn Smith.) A proposed international standard for staphylococcus antitoxin. *Bull. Hlth. Org.* **4**, 68.

1935. (With P. Bruce White.) A proposed international standard for gas gangrene antitoxin (vibrion septique). *Bull. Hlth. Org.* **4**, 13.

1935. (With Wilson Smith.) A proposed international standard for anti-pneumococcus serum (type II). *Bull. Hlth. Org.* **4**, 65.

1935. The effect of certain constituents of culture medium on the production of diphtheria antitoxin in normal and immunized guineapigs. *Brit. J. exp. Path.* **16**, 460.

1935. The effect, on the degree of immunity produced, of injecting the same dose of diphtheria formol-toxoid contained in different volumes of normal salt solution. *Brit. J. exp. Path.* **16**, 468.

1935. International biological standards. *Pharm. J.* **2**, 625.

1936. A simple laboratory method for the desiccation of serum and other protein solutions. *Bull. Hlth. Org.* **5**, 735.

1936. Report on the international biological standards maintained at the National Institute for Medical Research, Hampstead, London, on behalf of the Health Organization of the League of Nations. *Bull. Hlth. Org.* **5**, 713.

1938. Claus Jensen. *Lancet,* **1**, 335.

1938. The results of twenty different assays of five samples of tetanus antitoxin. *Bull. Hlth. Org.* **7**, 776.

1939. Anaphylaxis: passive sensitization *in vitro*. *Third International Congress for Microbiology,* p. 763.

1943. (With D. G. Evans.) Standard preparations for the assay of the three gas gangrene antitoxins, *Cl. perfringens, Cl. vibrion septique,* and *Cl. oedematiens*. *Bull. Hlth. Org.* **10**, 97.

1943. (With D. G. Evans and O. M. Hartley.) Factors affecting the response of immunized guineapigs to antigenic stimulus. *Lancet* **2**, 314.

1943. Discussion on active immunity. Part 1—General Considerations. *Proc. Roy. Soc. Med.* **36**, 147.

1945. Co-operative investigations relating to the assay of the potency of penicillin carried out for the international conference. *Bull. Hlth. Org.* **12**, 192.

1945. Memorandum on the international penicillin standard and the international penicillin working standard. *Bull. Hlth. Org.* **12**, 243.

1945. A world standard and unit for penicillin. *Canad. J. Publ. Hlth.* **36**, 196.

1945. Notes on the international standards for antitoxins and antisera. *Bull. Hlth. Org.* **12**, 76.

1945. International biological standards. *Brit. Med. J.* **2**, 579.

1945. Notes on the international standards for drugs, hormones and vitamins. *Bull. Hlth. Org.* **12**, 98.

1945. The effect of freezing on the potency of tetanus antitoxin. *Quart. J. Pharm.* **18**, 359.

1945. Diphtheria antigens—their preparation, properties, laboratory testing and statutory control. *Proc. Roy. Soc. Med.* **38**, 473.

1945. International biological standards: prospect and retrospect. *Proc. Roy. Soc. Med.* **39**, 45.

1946. (With D. G. Evans.) Discussion on the toxaemia of gas gangrene. *Proc. Roy. Soc. Med.* **39**, 295.

1946. Dr Louis Martin. *Nature, Lond.* **158**, 408.

1946. The importance of international biological standards in pharmacy. *Pharm. J.* **2**, 217.

1948. *Biological standards in science survey.* Ed. by Ian Cox, London: Sampson Low, pp. 263-265.

1948. (With T. Anderson, N. D. Begg, W. Gunn, R. Cruikshank, W. J. Martin and I. Taylor). Gamma-globulin in the prevention and attenuation of measles. *Lancet,* **2**, 41.

1948. Discussion on nutrition and resistance to infection. *Proc. Roy. Soc. Med.* **41**, 328.

1948. The behaviour of different types of homologous and heterologous diphtheria antitoxin when administered to pregnant guineapigs. *Mon. Bull. of Min. of Hlth. and Public Hlth. Lab. Service,* **7**, 45.

1948. In Emmens, C. W.—*Principles of biological assay.* London: Chapman and Hall, Foreword, *v.*

1949. Materials used in Great Britain for the active immunization of man against diphtheria. *J. Pharm. Pharmacol.* **1**, 425.

1949. *A survey of the British Pharmacopœia, 1948: biological substances.* The Pharmaceutical Press, p. 59.

1949. (With O. M. Hartley.) Factors affecting the response of immunized guineapigs to antigenic stimulus. *Lancet,* **1**, 1003.

1949. Chloromycetin. *Fight against disease,* **37**, 3.

1950. P. Bruce White. *J. Path. Bact.* **62**, 468.

1950. (With W. J. Tulloch, M. Anderson, W. A. Davidson, J. Grant, W. M. Jamieson, C. Neubauer, R. Norton, and G. H. Robertson.) A study of diphtheria in two areas of Great Britain. *Special Report Series,* No. 272 (Medical Research Council), and (summary of this Report) *Mon. Bull. of Min. of Hlth. and Public Hlth. Lab. Service* (1951), **10**, 143.

1951. The effect of peptic digestion on the properties of diphtheria antitoxin. *Proc. Roy. Soc.* B, **138**, 499.

1952. Dr H. D. Dakin, F.R.S. *Nature, Lond.* **169**, 481.

1952. Henry Drysdale Dakin. *Obit. Not. Roy. Soc.* **8**, 129.

1953. H. S. Raper. *Obit. Not. Roy. Soc.* **8**, 567.

Louis V. King.

LOUIS VESSOT KING

1886-1956

GIFTED inventor and foremost mathematical physicist in Canadian history, LOUIS VESSOT KING possessed an unusual range of abilities. His exceptional powers as a mathematician with a flair for elegance were accompanied by a profound knowledge of the physical problems invariably under consideration and even by an ability in detailed design expected only of engineers. In many fields of physics his natural gifts and knowledge were demonstrated, particularly in inventions and in suitable and elegant approximations.

Son of Alonzo King, M.A., and Louisa King (*née* Vessot), Louis was born in Toronto on 18 April 1886. His father was a schoolmaster who spent some intervening years at Stanstead Academy and at Hamilton, Ontario; so that Louis was eight years old by the time the family settled in Montreal. Alonzo King died at a relatively early age, leaving a very capable widow with her two children, Louis and Mabel. The family lived for many years in the stone house on Ste. Famille Street—where Mabel still lives—only a short distance from the house occupied for a time by Professor Ernest Rutherford and his family.

Louis inherited strong traits from both sides of the family. He used to say he had a French soul in an English suit of armour. His grandfather, Joseph Vessot, of Maçon, France, after a short term in the French Army, became an effective leader in the spread of Protestantism in Canada under the auspices of the Presbyterian Church of Canada. The Reverend Joseph Vessot finally settled in Joliette and in 1852 founded there—in the heart of French Canada—a vigorous Presbyterian Church. Perhaps the most significant related fact, however, was the cordial relations which eventually existed between this remarkable clergyman and the Roman Catholic people in the community.

With this background, it is not surprising that Louis was a Protestant with a deep appreciation of spiritual values. That a light touch was not lacking in the Vessot family, however, was indicated by his mother during a recital of some incidents in Louis's early life. When she recalled that 'Louis attended Sunday School regularly', she was asked, 'Do you think it did him any good?' 'I don't know about that', replied Mrs King, 'but I know it was good for me.'

As a boy, Louis was fascinated by the details of design and manufacture at the iron works operated by his uncle Samuel Vessot in Joliette. His interest in craftsmanship was further developed at home where he had a room equipped with tools received as birthday and Christmas gifts.

His lasting appreciation of many aspects of country life was developed during vacations spent on a large Ontario farm owned by his grandfather, James King. Louis became very fond of animals—particularly dogs and horses—and he was a good rider.

Mrs James King (*née* McKenzie) was a member of a family that emigrated from Edinburgh to Canada. Her brother, Malcolm McKenzie, made a most remarkable record as a law student at Victoria College, Toronto. This brilliant young man was one of the few Canadians killed in the Fenian Raids. The tragic loss to Canada is indicated by the erection of an obelisk at Woodstock, Ontario.

This record of high achievement under pioneer conditions had a strong influence on Louis which he proudly acknowledged. It must be added, however, that in the home an energetic mother encouraged every good interest. Indeed, the vivacity, natural charm and culture of Mrs Alonzo King stimulated lively discussions in her completely bilingual home, and friendships thus formed with distinguished visitors were eventually enriched as the exceptional gifts of both her son and her daughter were realized.

At the Montreal High School Louis was always the youngest member of his class and always received the highest honours and awards. This standard arose not only from his exceptional abilities but also from his habit of studying beyond the range of the curriculum. While still a boy, he took Ganot's *Physics* to bed with him in order to satisfy his interest in a more complete presentation of elementary physics.

In 1901 he graduated from High School and entered McGill University; during his undergraduate years physical science was rapidly expanding at McGill due to the separate efforts of Professors Barnes and Rutherford. Although proof of the disintegration of the 'atom' came in the middle of his course and provided a rare scientific stimulus which was supplemented by a deep friendship between Rutherford and King, it was Barnes who had the greater influence on his early experiments and indeed on his life work, in which radioactivity does not appear.

In those historic years young King demonstrated talents so exceptional that upon graduation he was advised to try the Mathematical tripos at Cambridge. A special scholarship was awarded for the purpose and he entered Christ's College in the autumn of 1905.

Although his letters to the family indicated that in his opinion his talents might have been adequately developed by the solution of a smaller number of problems, King loved Cambridge. The new friends, an additional interest in good music, rowing for his College, Paris in vacation time, these were his new joys. Upon his return to Montreal, he and his sister joined in piano duets.

King immediately was given an appointment at McGill University and was rapidly advanced in academic rank. He was appointed demonstrator in physics in 1909, lecturer in 1910, assistant professor in 1913, associate professor in 1915 and finally Macdonald Professor of Physics in 1920. Thus at the early age of thirty-four years he held the position already made famous by Callendar, Rutherford, and Barnes.

King was at his best when he combined invention with mathematical analysis and put the whole affair on a practical working basis. Such was the case with his hot-wire anemometer and his tunable diaphragm for fog-alarm systems. He found much pleasure in providing elegant solutions to problems already solved by distinguished scientists as, for example, the mutual induction between two coaxial circles previously treated by Maxwell and by Gauss.

By inclination, King retained strictly classical methods of analysis. Over a short period, indeed, he made a rather determined effort to interpret the quantum theory in classical terms.

Most successful as an independent research worker, King had only a few post-graduate students. For some years he hoped to expand his activities through the establishment of what he called a mathematical laboratory. In this laboratory he planned to train students in the use of advanced practical mathematics and approximations. Thus a group of post-graduate students might be formed with training and interests similar to his own.

In the early autumn of 1938 King felt the approach of a recurrent mental illness and said to the writer 'My work at McGill is finished'. During the remaining years of his life, indeed, he did no scientific work. Tragically enough, one year after his retirement war came: and then King and his students would have been very welcome and very useful. During the first year of the war one often heard the remark, with reference to microwave problems, 'Here is where Louis King might have come in.'

Scientific work

Convection of heat from a cylinder of any form of cross-section in a stream of fluid was examined by King in theory and in experiments.

The general theoretical problem was solved through Boussinesq's transformation and calculation of heat loss per unit length was made under an assumed constant flux over the boundary. King discussed the hypothesis of constant flux in terms of his experience and also the question of temperature very near the cylinder.

The supporting experimental investigation was conducted in the exhaustive manner introduced at McGill by Callendar and Barnes. A small wire was set up in a fork and rotated with axis of rotation parallel to the wire. Correction of velocity for 'swirl' was experimentally determined in an ingenious manner. While the local background of knowledge of the behaviour of platinum wire in thermometers was useful, a 0·25 per cent error persisted in the temperature as determined by resistance. The radial temperature gradient was found to be negligible, but corrections were made for the cooling effect of leads and (when necessary) for the vibration of the wire. King's theory suggested that the velocity might be determined from $\overline{W}$, the average heat loss (observed by electrical measurements) through the relation

$$\overline{W} = B\sqrt{V} + C$$

where V is the velocity of the air; B and C are functions of temperature and dimensions of the wire. By plotting W against $\sqrt{V}$, the observed family of approximately straight lines, corresponding to the chosen temperatures, a check was made on the determination of B and C. Finally, details were provided for a portable hot-wire anemometer.

The practical aspects of his anemometer were presented by King in the *Journal of the Franklin Institute.* In appreciation of the quality of this publication, the Institute awarded to King a Howard N. Potts Gold Medal 'For his improved method and researches in hot-wire anemometry'. This award was all the more remarkable since a like Medal was received by Dr A. E. Kennelly of the Massachusetts Institute of Technology for his work in the same field.

During the First World War King invented a continuously tunable diaphragm for sending and receiving underwater sound. Earlier experience with existing equipment had indicated the need for a tunable device. This work he published with complete theoretical and experimental details.

A heavy central portion of the three inch circular diaphragm—intended to be rigid—was supported by a relatively light and flexible annulus tapered from the inner radius, r, to the outer radius, $2r$. The one continuous piece of steel included a very heavy outer rim skilfully fitted to a heavy bronze mount which closed the system airtight. Tuning was realized by increasing (or decreasing) the gas pressure in the enclosure, thus placing the annulus under tension and increasing the natural frequency. This method of tuning was tested by using the diaphragm as part of a simple microphone receiver immersed in a tank of water inside a ship. It was learned that the natural frequency was readily changed by 50 per cent. Many of these diaphragms have been used in submarine fog-alarm systems set up in Canadian and U.S. waters.

Soon after his return from Cambridge, King developed, on classical grounds, the general integral equation for radiation scattered by a uniformly illuminated gas and included in his analysis the effect of self-illumination, i.e. the aggregate radiation reaching each element from all other gas elements. An approximate solution was found to apply to sky radiation observed on a flat earth. The expression for intensity of radiation of given wavelength as viewed in any direction at any time includes terms allowing for attenuation of incident and scattered radiation, attenuation by absorption alone and the contribution from self-illumination. This last radiation is largely unpolarized and King considered it mainly responsible for the lack of complete polarization of sky radiation. The closing analysis of the observations made by others at Washington and Mount Wilson makes this work a standard of reference. A contemplated analysis of effects in the atmosphere of the sun intended to provide understanding of the total departure of observed sunlight from black-body radiation was not carried out.

Ten years later, Professor King gave in classical terms a general dispersion theory in which he assumed a complex anisotropic molecule whose numerous principal directions of elementary vibrations were not parallel. Expressions for the intensity of light scattered at any specified angle with reference to the incident beam and the corresponding coefficient of extinction were found to be invariant with respect to details of molecular structure.

Field work in geophysics by Eve and Keys suggested to King the following problem, viz. to learn the nature of strata from the observed potential gradient between two point conductors on the free surface. For the purpose of analysis, King assumed that the strata were plane and of uniform thickness or, in other words, that the electrical conductivity was a function of depth only. He recognized this as the fundamental problem of finding a solution for a single electrode from which current flows to infinity. A current function is found in terms of which surface potentials and gradients may be easily derived and the solution for practical conditions involving more than one electrode are found by addition. The current function and related quantities were worked out in detail for several special cases.

The ultra-sonic radiation field from a rigid circular disk moving with constant amplitude in an infinite rigid and closely fitting wall was examined in detail by King with particular reference to sea water. The velocity potential was expressed as an infinite integral involving cylindrical co-ordinates. It was learned that viscosity has little effect on the field near the disk, but causes a marked limitation in range. For given oscillator output and range required, there is an optimum wavelength which may be read from charts provided.

A study of electromagnetic shielding in the kilocycle range enabled King to calculate shielding ratios for thin conducting shields and these he carried out for spherical and open cylindrical shapes. The shape proved to be relatively unimportant, but material of high permeability proved very effective at the higher frequencies. Even so, shielded radio transformers will induce currents in the soft iron shield and lead to appreciable hysteresis losses. In this case King concludes, the iron should be protected by an inner non-magnetic shield of low resistance.

The last paper by King dealt with the characteristics of a perfectly conducting cylindrical antenna insulated from a perfectly conducting flat earth. His introductory and concluding remarks are quoted below.

'The art of radio-broadcasting has in recent years directed attention to the radiation characteristics of the vertical antenna in circumstances where the height is comparable to the wavelength λ. Existing theory generally assumes a sinusoidal distribution of current along the antenna. By integrating Hertz's expression for the radiation field of a current element it is indeed possible to obtain expressions for the electric and magnetic vectors of the entire antenna, and ultimately, by making use of Poynting's theorem over a very large hemisphere, to arrive at the well-known expression for the radiation resistance. Such an antenna, known as a sine-wave antenna, gives results in tolerably good agreement with observation as long as l/λ does not exceed $1/4$. This theory, admittedly imperfect, predicts sharp nodes of current along the antenna when $l/\lambda > 1/2$, with the result that the radiation resistance calculated with respect to the current at the base of the antenna tends to infinite values at $l/\lambda = 1/2, 3/2, 5/2 \ldots$, etc. Then again the theory of the sine-wave antenna ignores a prescribed boundary condition over the surface of the conductor. In the case of a perfectly conducting antenna this requires that the component E_z of the electric field along the antenna should be zero over a cylinder of radius $\rho = a$. The sine-wave solution, however, gives rise to two components of E_z in phase quadrature, neither of which is zero along the antenna, while one of them tends to infinite values at the foot and at the top of the antenna.

'Careful measurements of current distribution along an antenna of uniform cross-section have recently been carried out by Professor P. O. Pedersen (1935) at Copenhagen. His results for $l/\lambda = 0{\cdot}581$ show that the root mean square of current is approximately sinusoidal, but shows a minimum instead of a zero value at the node anticipated by the theory of the sine-wave antenna.

'Many attempts have been made to improve the theory of the antenna by the use of the familiar transmission-line formulae. Results obtained by this means are admittedly imperfect and incorrect in principle, since the ideas of self-induction and capacity per unit length of conductor are based on the existence of electrostatic and magnetic potentials. When the dimensions of the antenna are comparable with the wave-length, such potentials must be replaced by wave-potentials, and in these circumstances it is difficult to estimate the accuracy of line-transmission calculations.

'It might be thought that from the measured current distribution it would be possible by integration of the field due to each current element, to arrive at expressions for the electric and magnetic vectors, and hence at an evaluation of radiation resistance. It turns out, however, on examination of the problem according to the electromagnetic field equations, that prescribed boundary conditions along the antenna lead to two components of antenna-current in phase quadrature, the relative amplitudes of which vary with distance from the earth. Measurements, however, are only capable of giving the root mean square of these components. Unless the two components can be resolved separately by more refined observations, the method of integrating the root mean square of current is theoretically incorrect and cannot be expected to give more than somewhat rough results.

'It will be seen from this brief review of the situation that a solution of the antenna problem in terms of the electromagnetic field equations only, with due regard to the boundary conditions along the conductor, is desirable. The solution of the problem as developed in the following sections is found to depend on an integral equation. A first approximation of sufficient accuracy to be of interest as a practical solution, is obtained by analytical methods. Second and higher approximations, however, lead to refractory integrals whose evaluation is only possible by quadratures or methods of machine integration.'

* * * * * *

'Needless to remark, the results of this paper and the antenna characteristics will be considerably modified by taking into account the effect of an imperfectly conducting dielectric earth and to a much lesser extent by the ohmic resistance of the conductor. The writer has considered it of some importance to have available as a standard of reference the radiation characteristics of the perfectly conducting antenna over a perfectly conducting earth, as several features of its behaviour formerly ascribed to earth currents turn out to be inherent in the solution of the electromagnetic equations with due regard to the boundary conditions along the conductor.'

In 1930 King married Miss Dorothea Neville, a professional nurse who was wholly devoted to her husband. Her protection against disturbing influences permitted him to work much more effectively in succeeding years. This beneficial influence of Mrs King is indeed apparent in the following bibliography.

On social occasions, visitors of all classes found their home attractive. Kind and thoughtful of others, King was a good listener, generous in his judgments, a great man. In conversation, he could contribute original viewpoints to a wide range of subjects—usually with much penetration and good humour. His interest in music was maintained and he occasionally entertained his friends at the piano.

An immortal among Canadian scientists, Louis King passed away rather unexpectedly on 6 June 1956, following a surgical operation. News of his death was received with much sadness by his many friends who recalled his personal charm and great achievements.

He is survived by his wife, Mrs Dorothea King, of Guelph, Ontario, and his sister, Miss Mabel King, of Montreal. The writer is much indebted to these ladies for assistance in the preparation of this memoir.

J. S. FOSTER

SELECTED BIBLIOGRAPHY

1913. On the scattering and absorption of light in gaseous media, with applications to the intensity of sky radiation. *Phil. Trans.* A, **212**, 375-433.

1914. On the convection of heat from small cylinders in a stream of fluid: determination of the convection constants of small platinum wires with applications to hot-wire anemometry. *Phil. Trans.* A, **214**, 373-422.

1917. On the mathematical theory of internal friction and the limiting strength of rocks under conditions of stress existing in the interior of the earth. *J. Geol.* **25**, 638-658.

1918. Improvements in and relating to the tuning of diaphragms for generating or receiving sound waves. *British Patent Specifications,* 131,041: U.S., 1,375,707.

1919. On the determination of electrical and acoustic characteristics of telephone receivers. *J. Franklin Inst.* **187**, 611-625.

1921. On some new formulae for the numerical calculation of the mutual induction of coaxial circles. *Proc. Roy. Soc.* A, **100**, 60-66.

1922. On a lecture-room demonstration of atomic models. *Phil. Mag.* **44**, 395-400.

UNIVERSITY COLLEGE
GRAHAM LIBRARY
HOSPITAL MEDICAL SCHOOL

1923. On the complex anisotropic molecule in relation to the dispersion and scattering of light. *Proc. Roy. Soc.* A, **104**, 333-357.

1924. *On the direct numerical calculation of elliptic functions and integrals.* Cambridge Univ. Press, viii + 42 pp.

1926. Characteristics of continuously tunable diaphragms, *J. Sci. Instrum.* **3**, 241-245.

1933. Electromagnetic shielding at radio frequencies. *Phil. Mag.* **15**, 201-223.

1933. On some new formulae for the calculation of self and mutual induction of coaxial circular coils in terms of arithmetico-geometrical scales. *Phil. Mag.* **15**, 1097-1114.

1933. On the flow of electric current in semi-infinite stratified media. *Proc. Roy. Soc.* A, **139**, 237-277.

1934. On the flow of electric current in semi-infinite media in which the specific resistance is a function of depth. *Phil. Trans.* A, **233**, 327-359.

1934. On the acoustic radiation field of the piezo-electric oscillator and the effect of viscosity on transmission. *Canad. J. Research* **11**, 135-155. Part II *Canad. J. Research,* **11**, 484-488.

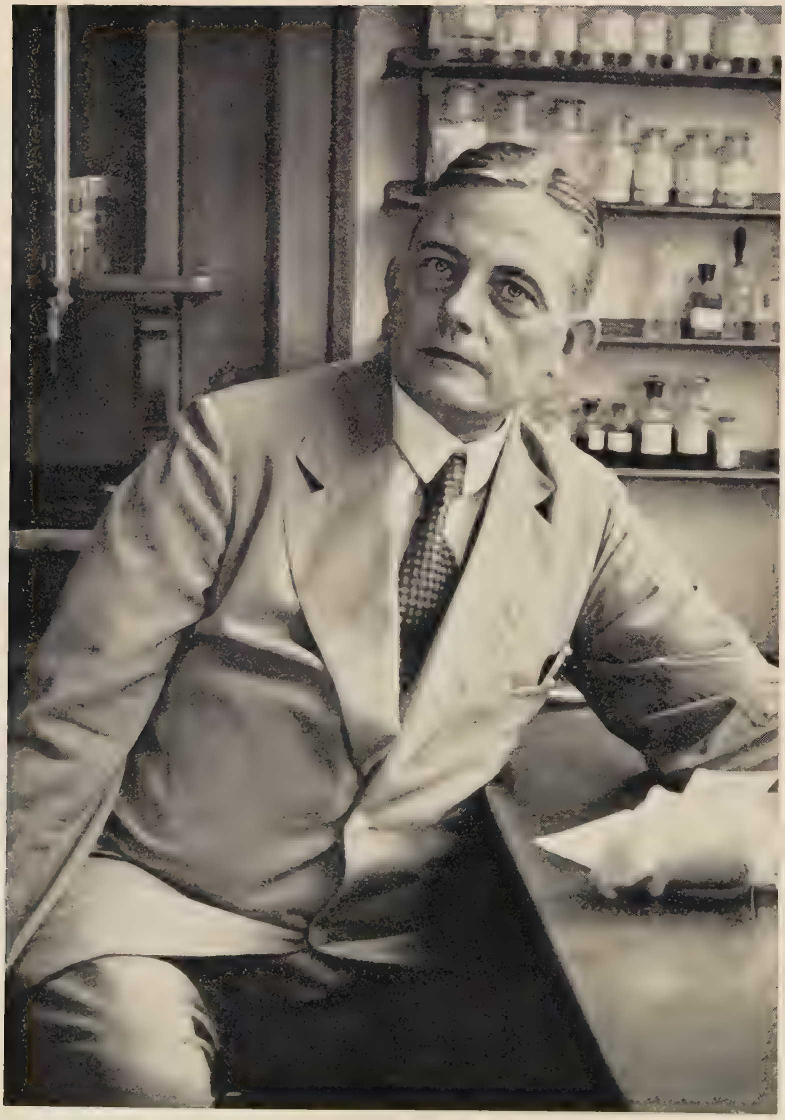
1934. On the acoustic radiation pressure on spheres. *Proc. Roy. Soc.* A, **147**, 212-240.

1935. On the acoustic radiation pressure on circular discs: inertia and diffraction corrections. *Proc. Roy. Soc.* A, **153**, 1-16.

1935. On the theory of the inertia and diffraction corrections for the Rayleigh disc. *Proc. Roy. Soc.* A, **153**, 17-40.

1936. On the electrical and acoustic conductivities of cylindrical tubes bounded by infinite flanges. *Phil. Mag.* **21**, 128-144.

1937. On the radiation field of a perfectly conducting base insulated cylindrical antenna over a perfectly conducting plane earth and the calculation of radiation resistance and reactance. *Phil. Trans.* A, **236**, 381-422.



AJKluyver

ALBERT JAN KLUYVER

1888-1956

Albert Jan Kluyver was elected a Foreign Member of the Royal Society in 1952. Later in the same year he delivered the third Leeuwenhoek Lecture on 'The changing appraisal of the microbe' and only a year afterwards he was awarded the Copley Medal of the Society. This rapid succession of honours sufficiently emphasizes the esteem in which he had come to be held by microbiologists and biochemists in this country. He had more than maintained the unique position in general microbiology established by Beijerinck for the Delft laboratory, but he was above all a pioneer in investigations of the biochemistry of micro-organisms. It is for this latter aspect of his work and its repercussions on the advance of both biochemistry and microbiology that he will be best remembered.

Career and personal details

Albert Jan Kluyver was born on 3 June 1888 at Breda in the Netherlands and was the son of Jan Cornelis and Marie (born Honigh) Kluyver; his father was Professor of Mathematics in the University of Leyden. His schooldays were spent in Leyden and in 1905 he began his advanced education (in chemical engineering) at the Technische Hogeschool in Delft; as it turned out he was to spend the greater part of his life in the Microbiological Laboratory of this University.

The Microbiological Laboratory had been especially established in 1895 for M. W. Beijerinck in the then Polytechnical School, and was attached for convenience to the division of chemical technology. Beijerinck was the first Professor of General and Applied Microbiology. In 1905, Kluyver's first year as a student, the Polytechnic was raised to University status and by 1907 there was some expansion on the biological side with the result that Dr G. van Iterson (a pupil of Beijerinck) was appointed Professor of Microscopical Anatomy. There was clearly a close liaison between chemistry and microbiology in Delft in Kluyver's student days, and this attachment remains until the present time.

Kluyver graduated with distinction in chemical engineering in 1910. During the previous year he had been a student in van Iterson's laboratory and had made an excellent impression. The Professor felt the need to bring chemistry to the aid of his work and immediately appointed Kluyver to an assistantship. This was a great success, for Kluyver proved to be as excellent and enthusiastic a teacher as a research worker. He held this post until 1916 with two breaks—a short period studying plant physiology under Molisch in Vienna (which incidentally led to his first published papers) and a year in the Dutch army at the outbreak of the first world war. During this period he made his first contribution to chemical microbiology, a thorough investigation of the ability of various yeasts to ferment different sugars. The research also led to accurate biological methods for the determination of individual sugars in mixtures—a very difficult task by other methods before the recent introduction of chromatographic techniques. Beijerinck is said to have been much impressed by this work which gained for Kluyver in 1914 his degree of Doctor of Technical Science 'with distinction'.

In 1916 began another association which was to bring Kluyver lifelong happiness; he met and married Miss H. J. van Lutsenburg Maas. She was a biologist and was also working at the time in the Department of Technical Botany in Delft. They had two sons and three daughters and their life together is always described by those who knew them intimately as ideally happy. Her death in 1952 was a severe blow which greatly saddened his last years.

Like many Dutch scientists at the time, Kluyver spent some years in the (then) Dutch East Indies. In 1916 he was appointed, on van Iterson's recommendation, as a consultant for the Dutch East Indies to the Department of Agriculture, Industry and Commerce. The stay broadened his biological outlook and increased his flair for recognizing possible practical applications of pure research. Part of his duties was to investigate the copra industry in Ceylon and the Malabar Coast, and to determine the reasons for the successful extraction of coco-nut fibre in these regions; if this could be applied in the East Indies, the Dutch colonies might obtain part of the world market.

Kluyver left the Indies in the winter of 1919-20 but soon returned as head of the research laboratory in Bandung of Insulinde Ltd, a firm dealing in vegetable oils. But the prospects of the firm were adversely affected by the financial crisis of 1920; from the point of view of science this proved to be a fortunate chance and a turning point in Kluyver's career. For in 1921 Beijerinck retired from the Chair of General and Applied Microbiology and from the directorship of the laboratory which he had built up to a position of world renown. In 1922, strongly supported by van Iterson, Kluyver was chosen to succeed Beijerinck at the early age of 33; he occupied the Chair with ever-increasing distinction for the rest of his life.

The selection of Kluyver must have seemed somewhat of a gamble at the time. He had comparatively little knowledge of general microbiology and had the unenviable task of succeeding a man who was an acknowledged world leader in research, and an inspiring teacher. But the faith of his sponsors was well placed and the gamble, if it was one, succeeded brilliantly. Initially this was no doubt partly due to the fact that Kluyver realized his own limitations only too well, and had accepted the post only after much hesitation and with a determination to make good what he thought to be his deficiencies. His inaugural lecture was cautious and centred on the economic possibilities of the use of micro-organisms. There followed a remarkable achievement of concentration in which in a very short time he expanded his knowledge of microbiology in all its aspects and at the same time provided himself with that survey of the almost infinite variety of mode of life (and, therefore, metabolic activities) of micro-organisms which was to be the basis of most of his future research. As he put it many years later in the Leeuwenhoek Lecture 'For the moment the most important conclusion is that—with few exceptions—there is no set of conditions realized on earth which does not answer the requirements of some specific form of microbe. In other words the ubiquity of microbes on earth reveals the almost infinite capacity of life for adapting itself to a practically endless variety of external conditions.'

At all events the lag phase was very brief and within a year or so active and fruitful research by Kluyver and his associates was in full swing. Kluyver acknowledged freely and with gratitude the great help he received in the early days on the more purely microbiological side from his colleague Dr L. E. den Dooren de Jong. The period 1924 to 1939 was one of exponential growth both of Kluyver's powers and the output of his laboratory. The work will be reviewed later. By the mid nineteen thirties Kluyver had attained an international reputation, and his laboratory had become a recognized centre of research in chemical and general microbiology which attracted research workers from all over the world.

From 1935 onwards Kluyver also undertook the joint direction of a team of workers (the Biophysical Research Group Utrecht-Delft) researching on problems of photobiology. This enterprise was made possible by the aid of the Rockefeller Foundation and Kluyver's co-director was initially a well-known spectroscopist, Professor L. S. Ornstein, who held the chair of Physics in Utrecht. The Group has made distinguished contributions to knowledge of bioluminescence and photosynthesis, the luminous and photosynthetic bacteria being extensively used. The work of the Group, and Kluyver's part in it, has been reviewed by E. C. Wassink in the obituary notice written by Kingma Boltjes and Wassink (1957).

The second world war brought the grand phase of Kluyver's career, then at its climax, to an abrupt end. After the surrender of the Netherlands work in his laboratory became impossible and virtually ceased. A little secret work on penicillin production later proved helpful to Dutch industry. Kluyver was under severe strain and was left mentally and physically exhausted. Conditions in the immediate post-war years were not conducive to the resumption of work on a large scale, and Kluyver himself felt out of touch and depressed by the bulk of the literature that had been published. But in spite of other duties Kluyver's unique powers of concentration and assimilation were clearly still with him. One has only to read his three John M. Prather lectures to Harvard University, and particularly the one entitled 'Life's flexibility', to realize how detailed and firm was his grasp of modern developments.

Time available for personal research was limited after the war by the increasing number of general and administrative tasks which came his way, all of them reflecting the value attached to his advice and influence. During this period he served as Rector Magnificus of his University and as President of the Natural Science Section of the Koninklijke Akademie van Wetenschappen of the Netherlands. Much time had to be spent on the planning of a new and larger Microbiological Institute in Delft, made necessary in a large part by the reputation Kluyver's own work had built up; this was a labour of love for microbiology since, even if he had survived, Kluyver would only have been able to occupy the new laboratory for a year before he had to retire.

The honours bestowed upon him, both in his own country and abroad, often meant the preparation of special lectures. Indeed, Kluyver's capacity for picking on the essentials after a comprehensive survey, and reaching the heart of a problem, caused him to be in great demand as a lecturer. He took such duties very seriously, and went to infinite pains to make the lecture or speech really fit the occasion, even if it meant much study in unusual fields. A remarkable example of this is his account of the early Dutch settlements in the U.S.A. given at the opening ceremony of S. A. Waksman's new Institute of Microbiology at Rutger's University.

Apart from the honours already mentioned, many learned societies were eager to elect Kluyver to honorary membership. These included our own Society for General Microbiology, the Society of American Bacteriologists, the Flemish Royal Academy of Sciences and the Academy of Sciences of the U.S.A. Honorary doctorates came from Zurich, Rutgers and Iowa State College (where he spent three months as a visiting professor in 1932). Denmark honoured him by the award of the Emil Christian Hansen medal in 1946 and his own country made him a Knight of the Order of the 'Nederlandse Leeuw'.

Colleagues and former pupils of Kluyver all speak of his warm humanity and never-failing courtesy. It is clear that everyone who worked in his laboratory became devoted to him. It would be impossible for a comparative stranger to do justice to this aspect of the man, but the writer clearly remembers his own first meeting (as a very junior research worker in the early 'thirties) with Kluyver and the kindly interest and deep attention he gave to the matters we discussed: he gave then a sense of friendship and of collaboration to our relationship which was maintained to the end in all too rare meetings. Excellent accounts of Kluyver as a man and as a teacher have been given by van Niel (1957), van Iterson (1939) and Kingma Boltjes (1939). His courage and unwillingness to let anyone down are exemplified by the occasion of his Leeuwenhoek Lecture. He was injured and severely shaken by an accident on the boat in rough weather on the way over, yet he refused to consider postponing his lecture.

Kluyver worked exceptionally hard throughout his life. He was accessible to his colleagues and students and ready with advice and encouragement at all hours of the day, and for a good deal of the night as well. For his house adjoined the laboratory and there was a communicating door; it is characteristic of Kluyver that he felt this essential for a head of a department and insisted on a similar arrangement in the plans for the new Institute. The meticulous care given by Kluyver to the preparation of scientific papers and the Dissertations of his pupils has become almost a legend.

No one reports that Kluyver had any hobbies. His life was consecrated to microbiology in its broadest sense; yet this induced no narrowness of mind or indifference to human problems. To such a man the thought of retirement must have been abhorrent. During his last years he suffered from angina pectoris: yet he seemed to take little interest in his health, took almost no precautions, and maintained the intensity of his work until the end. After working as usual until after midnight he died as a result of a heart attack early in the morning of 14 May 1956.

Scientific work

Kluyver's work will not be remembered for individual experimental discoveries of major importance, of themselves opening up new fields of investigation. His genius lay in another direction—the detailed and accurate amassing of information, the penetrating and critical analysis of the results, and the final perception from many such studies of underlying principles which were woven into working hypotheses to be supported (or not) by further investigations based upon them. The very bulk and thoroughness of the work makes it difficult to point to highlights; almost everything he did contributed something to the basis of the great generalizations with which his name is associated. A complete bibliography will therefore be given, and no attempt will be made to analyze in detail all aspects of his work. For the same reasons it is scarcely possible to separate personal achievements from those of individual pupils; what is quite certain is that the latter are always the first to recognize the inspiration they received from Kluyver. A list of Dissertations (for Doctor's degrees) based on work carried out in Kluyver's laboratory during his professorship is also appended.

The continual underlying theme of Kluyver's broad investigations into the biochemistry and biology of micro-organisms was the search for common factors. This led him ultimately to the enunciation of the principle of the basic similarity in biochemical detail of many processes fundamental to the life of diverse types of micro-organisms, and indeed all organisms. As a natural consequence, since he knew that the biochemical potentialities of microbes covered a wider spectrum than the animal and plant kingdoms put together, came his realization of, and propaganda for, the value of comparative biochemistry (a term he introduced in 1930) as a tool in general biochemical investigation. As he put it in his Leeuwenhoek lecture 'It will also be clear that it is exactly the surprising diversity in metabolic achievements of the microbe world which opens unique opportunities for a trend of study which may be called comparative biochemistry. The ultimate aim of this study should be the discovery of the unitarian principle which underlies the diversity in phenomenon'.

It is doubtful whether the present generation of biochemists realizes how much they owe to Kluyver. Yet one has only to skim through a recent volume of any biochemical journal to discover the extent to which the comparative approach is used and how frequently the microbe is turned to for aid in the solution of general problems, be it initial clues as to the function in cellular metabolism of a new vitamin-like substance, or perhaps as the source of a particular enzyme. Kluyver was also one of the first to speak of the experimental convenience of work with microbes. The high metabolic rate and rapid multiplication, the possibility of rigid control of the conditions of growth, the ability in many cases to survive over a wide range of temperature, pH and so on, combine to give a latitude which permits types of experiment impossible with, say, animal cells. If some of Kluyver's generalizations seem somewhat obvious today it is only because they have been long since built into the framework of biochemistry; they must be set against the background of knowledge of twenty to thirty years ago.

There is even a danger these days of the concept of similarity in basic biochemical processes of the cell being carried too far, a danger of losing sight of the individuality of the organism itself. But Kluyver certainly did not, and would not, have fallen into this trap. He was not just a good chemist or biochemist who realized the value of microbes as tools. This was perhaps the case when he was a post-graduate student, but from the moment he succeeded Beijerinck in the Chair a second thread ran through his work growing always stronger; this was a deep interest in the microbe itself. He was not concerned with the metabolic reactions he studied merely as chemical tricks (however skilful and unexpected), but in their role in the life of the organism. The microbes he used were in the main isolated in his own laboratory by the methods made famous by his predecessor; he encouraged his pupils to subject them to a biological examination just as thorough as the biochemical one which was to follow. On this subject he once permitted himself a slight tilt at some of his colleagues: 'This leads to the situation that curators of culture collections are frequently troubled with enquiries . . . although it is most likely that the microbe in question is at the same time slumbering in the dust adhering to the shoe of the writer of the letter. Evidently such microbiologists are not so much in want of a microbe as of a better command of their science.'

The time at which Kluyver began to work with micro-organisms (1909) was one by which (in his own words) 'The principle of the elective culture had opened the world of invisible life.' By then the initial period of exploration and isolation was slackening, and in 1922, when he came to succeed Beijerinck, the time was ripe for a deeper analysis. The almost bewildering variety of modes of life to be found implied an equal variety of metabolic activity. With his chemical background and analytical mind the particular challenge to Kluyver was to find the underlying similarities in metabolic processes. His first ten years or so in the Chair were devoted in the main to this end and by that time his concept of unity in biochemistry was well established. It was born after only three or four years, but it was Kluyver's way to enunciate his general views based on sound, but limited, experimental evidence, and then to seek confirmation and expansion in a very wide and detailed investigation with many organisms.

Kluyver had been impressed by work on glucose fermentation by yeast in the pre-1914 period, and particularly that of Neuberg, which had led to this obviously complex process being envisaged as a stepwise series of relatively simple chemical changes. If this were a common feature of the metabolism of micro-organisms then here was clearly a first simplification which might be of aid in the search for similarities. The first confirmation he obtained himself came in the field of oxidative rather than fermentative metabolism; it resulted from a chance observation (with F. J. G. de Leeuw) that an aerobic beer organism deposited crystals of calcium 5-ketogluconate when growing on a medium containing glucose and calcium carbonate. The conversion was almost quantitative and the organism (called *Acetobacter suboxydans* because of the limited powers of oxidation) similarly oxidized a number of sugar alcohols and glycerol to the corresponding keto-compounds. Other species of *Acetobacter* were found to oxidize sugars more extensively; a general survey (F. Visser 't Hooft) led Kluyver to propose that glucose was catabolized in a series of steps through gluconic and ketogluconic acids. He further suggested that various species failed completely or partially at various stages, the preceding compound appearing as the end product.

Incidentally the possible use of *Acetobacter suboxydans* for the manufacture of specific keto-compounds was realized and a patent taken out. It is, of course, now widely used for the preparation of sorbose from sorbitol, a step in the chemical synthesis of ascorbic acid.

About this time, too, Kluyver and his colleagues A. P. Struyk, H. J. L. Donker and F. Visser 't Hooft made several studies of yeast fermentation. From the present point of view the most important observation was that when the fermentation was carried out in the presence of a hydrogen acceptor (such as methylene blue) part of the acetaldehyde escaped reduction to alcohol and gave rise to acetoin and 2, 3-butylene glycol. An excursion into the role of cozymase was not so successful; certain observations led them to the conclusion that the most important constituent was a heat-stable antiprotease which protected the enzymes from destruction; for a long time Kluyver was not convinced about the importance of low molecular weight activators of the fermentation process. Van Niel (1957) is of the opinion that this set-back was partly responsible for Kluyver's lack of enthusiasm for *in vitro* studies with isolated enzymes.

Meanwhile other pupils of Kluyver had begun to make detailed studies of the fermentation of sugars and other compounds by various bacteria. These included the propionic acid fermentation (C. B. van Niel), the butyl alcohol/butyric acid fermentation (H. J. L. Donker and J. B. van der Lek), fermentation by coliform bacteria (M. A. Scheffer) and the fermentation of glycerol (H. R. Braak). The knowledge obtained combined with his own work led him towards certain general conclusions and formed part of their basis and later their confirmation. At the same time Kluyver's working hypotheses helped towards the success of these studies, which were characterized by most thorough analytical work and careful quantitative balance sheets from which arose reasonable suggestions as to the chemical pathways.

By 1926 Kluyver had reached tentative conclusions from the work of his own School and that of others; he then published (with H. J. L. Donker) the well-known 'Die Einheit in der Biochemie' in which he proposed that all catabolic reactions, aerobic and anaerobic, were a series of step reactions, and that furthermore all biochemical changes (apart from hydrolysis) could be envisaged as one or other of four types of inter or intramolecular hydrogen transfer. He was careful to emphasize that these views were to be regarded as a working hypothesis; the full fruition of the work in progress in his laboratory gave much support and by 1930 he was able to put forward a very strong case in his series of lectures to the University of London on 'The chemical activities of micro-organisms'. In these lectures also he first used the term 'comparative biochemistry' to describe this type of approach and advocated its value as a tool for biochemical investigations as a whole.

The merit of Kluyver's generalizations and methods, which greatly influenced biochemical work on micro-organisms, was two-fold. First, it gave great aid in unravelling catabolic processes; the complexity of different end products could be correlated with the nature and quantity of hydrogen acceptors available. Second, it brought into line the initial reactions of respiration and fermentation, thus reconciling the apparently opposing views of Wieland and Warburg on respiration. (Fleisch and Szent-Györgyi each independently reached the same conclusion at about the same time though on less broadly based information.)

With his interest in hydrogen transfer Kluyver naturally gave his attention also to those organisms which can live anaerobically with inorganic substances as final hydrogen acceptor for the energy-yielding oxidations; J. K. Baars made one of the typically thorough Delft studies of sulphate reduction by bacteria and the last major piece of research by Kluyver himself (with W. Verhoeven) was on nitrate reduction. In 1936 H. A. Barker studied methane fermentation in Delft and provided the first evidence for carbon dioxide acting as specific hydrogen acceptor and, at the same time, the first case of a definite and important function for carbon dioxide in heterotrophic metabolism. Later, Kluyver (with Ch. G. T. P. Schnellen) studied the production of methane from carbon monoxide; the latter reacted with water to give hydrogen and carbon dioxide, part of the carbon dioxide was then reduced by hydrogen to methane.

So far the experimental work of Kluyver's laboratory had been concerned with the catabolic or energy-yielding reactions. With his growing interest in the life and proliferation of the organism Kluyver had given much thought as to the possible mechanisms by which the energy was transferred to the synthetic reactions of the cell, but he did no work on synthetic pathways as such. He extended his unitary theory, and proposed a like stepwise sequence in anabolism, but his main concern was with the linkage to catabolism. He suggested (prophetically) that degradation might give rise to intermediates in which the energy was partly trapped, and which were also intermediates or reactants in the synthetic sequences, but he had no experimental evidence for this. Other workers (e.g. Cook and Stephenson; Barker) had observed that aerobic oxidation of substrates by suspensions of certain organisms did not give oxygen uptakes as great as would be expected for complete oxidation to carbon dioxide and water, although no end products could be found other than cell material. The possible close linkage here indicated between respiration and assimilation attracted Kluyver to take up the problem in his own laboratory. His pupil G. Giesberger made a comprehensive survey of the phenomenon with various species of *Spirillum*, whilst C. E. Clifton showed it was possible selectively to prevent assimilation and obtain theoretical oxygen uptake figures by adding dinitrophenol or sodium azide. Here then was the first mention of what have become known as uncoupling agents and are now known to prevent oxidative phosphorylation. (Dinitrophenol had been used for a time in the U.S.A. as a weight reducing agent for human obesity.)

In the early nineteen thirties a return was also made to problems of oxidative metabolism, this time with moulds. The work (with L. H. C. Perquin) now seems chiefly remarkable for the introduction of the submerged culture technique for the growth of moulds which was later to prove of such immense practical application in the manufacture of antibiotics. In studies of kojic acid formation by *Aspergillus*, Kluyver and Perquin were distressed by the difficulty of getting repeatable results even though conditions of growth were apparently the same. It seemed to them that a possible explanation lay in the difference in environment of different parts of the mycelium, partly aerobic (in the aerial mycelium) with low substrate concentration, and partly restricted in oxygen supply (in the submerged mycelium) with higher substrate concentration. They sought a method of growth in which the mycelium would all be in a similar environment; they found it by growing the mould in liquid medium with continuous agitation when the organism developed in small balls of tangled filaments throughout the medium.

The period 1930 to 1940 was partly one of consolidation and expansion of Kluyver's views on the unity of biochemistry and some of this work has been reviewed. It was also a period of great broadening of the general microbiological interests of the laboratory. Several students worked on mainly biological themes with bacteria and yeasts, and one with halophilic organisms. Biological and biochemical studies on autotrophic and photosynthetic bacteria became an important part of the total activity. Kluyver had always been fascinated by the particular problems of these modes of life; the necessity to bring about the overall reduction of carbon dioxide to that of cellular carbon brought them very much into his field of interest. The extensive researches of his pupils F. M. Muller and P. A. Roelofsen on photosynthetic bacteria form a most important background to modern work, while T. Y. Kingma Boltjes performed a similar service for the autotrophic nitrifying bacteria. Photobiology was also represented by several studies on luminous bacteria (K. L. van Schouwenberg, G. J. M. van der Kerk and A. van der Burg).

In his later years Kluyver took a personal part in several investigations of pigment formation by various microbes; indeed his last publication concerned the general physiology of a new species of pseudomonad (*Pseudomonas aureofaciens*) and the chemical nature of its pigments. It is difficult to trace the origin of this interest, unless perhaps it sprang from his earlier work (with B. Elema and J. C. Hoogerheide) on oxidation-reduction potentials in culture media; several bacterial pigments of course do undergo reversible oxidation and reduction.

It will have become obvious that throughout the years Kluyver and his colleagues studied a large number of micro-organisms of very diverse types. They were not content that this should be only at the biochemical level; whenever possible, and always with new or little-studied types, they made also a profound study of their general biology and physiology. In this way Kluyver came to have a very wide knowledge of the organisms themselves and it was natural that with his background and methods of approach he should seek underlying similarities on which to base an improved and more rational classification; it was equally natural that he should seek for aid in this from the physiological properties. In his lecture 'Three decades of progress in microbiology' Kluyver gives the credit to Orla-Jensen for introducing a new era in the study of bacterial classification by paying greater attention to physiological properties. Kluyver endorsed and extended these views in a practical manner, and, in collaboration with C. B. van Niel, published in 1936 the outline of a system of classification based on both morphological and biochemical characters.

Another great biological achievement of the Delft laboratory during the Kluyver regime concerned the yeasts. Beijerinck had left in the Institute a comprehensive collection of yeasts and Kluyver arranged to add to it the yeast cultures of the Centraalbureau voor Schimmelcultures in Baarn. The collection soon became the best in the world. Kluyver took advantage of this unique opportunity and initiated a policy of fundamental research coupled to the routine maintenance of the cultures; the result has been a series of classical monographs on the morphology, physiology, and classification of yeasts which are of the greatest value to all workers in the field. It is a great pity that Kluyver's enlightened policy with regard to such collections of micro-organisms is not more generally followed.

Although Kluyver worked in a technological university the greater part of his research was in the field of pure science. His early training gave him the ability to spot possible practical applications and he remained interested in the economic potentialities of the use of micro-organisms. Apart from specific instances already mentioned (e.g. the use of *Acetobacter suboxydans* in the conversion of polyhydric alcohols to keto-derivatives), the list of publications and Dissertations is lightly dotted with papers of a more applied nature; it is clear that Kluyver was not averse to using his profound knowledge of microbiology to this end. Basic matters of interest to the dairyman appear in studies of slime formation in milk, and the role of bacteria in the aroma and making of butter and in the souring of cream. In other fields he and his students investigated the efficiency of the chlorination of water and the process of filtering microbes from air (and the determination of microbes in the air). They also studied the causes of flax rust and the efficacy of fungicides in the preservation of wood. Finally (with E. H. Houwink) an attempt was made, by bacterial treatment, to modify rubber latex so that it vulcanized more uniformly. It will be recalled that Kluyver's inaugural lecture had dwelt upon the possible industrial applications of microbiology; in an address given almost thirty years later to the Microbiology Group of our Society for Chemical Industry he surveyed what had happened and predicted that in the future the main application in chemical industry would arise from the synthetic powers of microbes rather than from catabolic processes, and would be in the manufacture of large complex molecules or particular isomers of simpler ones.

A few more general matters related to Kluyver's work remain to be considered. Although his working life spanned the period of development and some of the major achievements of the study of isolated enzymes, he made almost no use of this method of approach to the solution of problems of intermediary metabolism. Indeed two of the only three papers bearing his name in which the word enzyme or the name of an enzyme appears in the title are (somewhat discouragingly): 'On the non-existence of some enzymes' and 'The so-called co-enzyme of alcoholic fermentation.' Yet Kluyver always believed firmly, and from the beginning, in the enzymic basis of metabolism. (It must be remembered that this was still under debate when he began work.) He often considered in some detail in connexion with 'unity in biochemistry' matters of enzyme specificity. Van Niel (1957) has discussed at length possible explanations for Kluyver's negative attitude towards enzymes. It seems to the writer that the clue may be found in Kluyver's devotion to the principle of comparative biochemistry. If he had used isolated enzyme technique he would undoubtedly have made some further progress with the elucidation of the details of his metabolic pathways and possibly have provided further support for his views on basic similarity of cell processes. But with the resources available he could have covered less ground; there was the ever-present challenge of groups of microbes with fascinating modes of life awaiting exploration at the biochemical level. Furthermore the main interest for Kluyver always seemed to be the integrated life of the organism. It is clear from his later writings that he appreciated the value of what has come to be known as enzymology: but he could have scarcely achieved more and had he entered this field, other work, which the Delft School could do better than anyone else, would not have been possible.

Kluyver greeted with enthusiasm the modern expansion of work on microbial adaptation and particularly that aspect which deals with the induced synthesis of enzymes. In the early days in his laboratory L. E. den Dooren de Jong had made a survey of the ability of some dozen common organisms to grow with any one of over a hundred compounds as sole carbon source in a defined medium with ammonia as the nitrogen source. Remarkable powers were shown by some organisms. Furthermore other organisms specially isolated from soil grew on even more substrates than members of the same genus already tested: *Pseudomonas putida*, for example, grew on 77 out of about 200 compounds tested, and amongst those giving positive results were synthetic compounds which the organism would certainly never meet in nature. At the time Kluyver was driven to the conclusion that the enzymes concerned with oxido-reduction reactions must be of low specificity since he felt it impossible to accept the view that specific enzymes were ready waiting in the organism to deal with such a variety of substrates. These ideas were out of line with current thought and were certainly not supported by later work on the dehydrogenases. L. E. den Dooren de Jong had also found that isolation of an organism from the soil in the presence of a given substance often enhanced the ability of the organism to use that substance. At the time Kluyver does not seem to have given this observation as much weight as he might, but when adaptive enzyme formation really came to the fore in the late 'thirties he quickly realized that it provided an excellent solution to his problem.

Kluyver's chief excursion into this field of adaptive enzymes was in the course of the last major piece of experimental work with which he was personally concerned. Other aspects of the research have been left until now because the work as a whole is not only his last main research, but seems to the writer to summarize and typify the whole of Kluyver's philosophy of approach and experimentation. It concerned the problem of denitrification, and was carried out with W. Verhoeven as main collaborator; others in the laboratory who took part in various aspects of the work were E. van Olden, J. J. C. Goos, A. L. Koster and M. C. A. van Nievelt.

The subject of microbial nitrate reduction attracted Kluyver because of his views on hydrogen transfer and its relation to anaerobic energy-yielding processes. Many bacteria were known to reduce nitrate to nitrite, ammonia or nitrogen, but Kluyver was mainly interested in what he and his colleagues termed *true dissimilatory nitrate reduction;* this he has defined in the sentence 'The full splendour of bacterial nitrate reduction, however, reveals itself in the case in which the nitrate is involved in an energy-yielding process which, at least conditionally, is essential for the well-being and proliferation of the organism'. He, therefore, confined himself to organisms which can actually grow with nitrate in place of oxygen as the ultimate hydrogen acceptor; this meant in practice the denitrifying organisms in which the product of nitrate reduction is nitrogen. For part of his work he sought an organism which could use molecular hydrogen itself as the donor or reducing agent: he had earlier worked (with A. Manten) on some aspects of the aerobic hydrogen oxidizing bacteria which are facultative autotrophs. He and his colleagues went to the soil and, by specific enrichment in an environment containing hydrogen, nitrate and carbonate, found such an organism. It did, however, require traces of organic growth factors. The organism turned out to be identical with *Micrococcus denitrificans* which had been isolated forty years earlier by Beijerinck and was still in the laboratory; its capacity to live an essentially autotrophic, anaerobic life had not been known.

Micrococcus denitrificans was able to grow heterotrophically with organic donors and either oxygen or nitrate as acceptor; it could also grow autotrophically with hydrogen as donor in the presence of either of the two acceptors. The organism therefore compassed a variety of modes of life; furthermore the extent and rate of growth was reasonably comparable under all conditions and the rate of oxidation of glucose by oxygen and nitrate was quantitatively similar under certain conditions. It seemed to Kluyver that the organism provided an ideal illustration of the trans-hydrogenation view of respiration which he felt was here 'almost tangible' because of the possibility of substituting the organic substrate by hydrogen.

It was early shown that the organism did not reduce nitrate unless grown in the presence of nitrate, that is that the enzyme activating nitrate was adaptive. Characteristically Kluyver was less interested in the mechanism of the adaptation than in its relation to the life of the organism. Further experiments with harvested suspensions showed that more than the mere presence of nitrate during growth was required; the organism also had to have been growing anaerobically, that is in conditions in which nitrate was essential. Similarly the enzyme activating hydrogen (hydrogenase) was found not to be formed unless both molecular hydrogen was present and the organism had been obliged to use it as a hydrogen donor for the growth process; the enzyme was formed when either oxygen or nitrate was the terminal acceptor, but it was not there if organic donors were available as well as hydrogen. (This is not true of other aerobic hydrogen bacteria). With *Micrococcus denitrificans* there is clearly a remarkable correlation between adaptation and the life of the organism.

Finally, with *Micrococcus denitrificans* and other denitrifying organisms, a thorough investigation was made of the steps in the overall process of reduction of nitrate to nitrogen. This was a typical Delft analytical study with excellent methodology and proper attention to chemical matters. It showed, first of all, that nitrous oxide was also a product and in some conditions quantitatively the major one. Nitramide, proposed by others as an intermediate, was found to be spontaneously decomposed. Hyponitrous acid, at any rate the isomer available, was excluded. No conclusion could be reached as to whether nitrous oxide was the immediate precursor of nitrogen as well as an end product.

Here then in a single research on one general topic is the isolation and general physiological examination of an organism with a fascinating mode of life, the recurrent themes of hydrogen transfer and stepwise simple reactions, the first class analytical and chemical work. And throughout all the insistence on the life of the organism itself as the thing that really matters.

Kluyver has left with us a record of service to microbiology which will be hard to equal. His experimental work, and that inspired by him, forms part of the foundations of chemical microbiology. But above all he has shown us how to combine microbiology with biochemistry in a way which adds lustre to both.

The author is deeply indebted to Professor C. B. van Niel for allowing him to see, in advance of publication, an appreciation of Professor Kluyver, and for permission to use information contained in it. Mr Carel Mulder gave great help by translating a number of papers. The assistance of Miss A. Pearce-Gervis in the preparation and checking of the Bibliography has been invaluable.

The photograph is reprinted by permission of the Editors and Publishers of 'Antonie van Leeuwenhoek' *Journal of Microbiology and Serology.*

D. D. Woods

BIBLIOGRAPHY

Section A is a list of Kluyver's own publications and is thought to be complete. It is based on those published by Perquin (1939) and Kingma Boltjes and Verhoeven (1956). Great assistance has been given by the Librarians of many libraries in this country and the Netherlands and especially by Dr J. W. M. la Rivière of the Technische Hogeschool, Laboratorium voor Microbiologie, Delft.

Section B is a list of Dissertations describing work carried out wholly or in part under Kluyver's supervision.

Section C gives the references to other biographical articles which have been quoted.

Section A

1911. De chemie van het chlorophyl in het licht van de nieuwere onderzoekingen. *Pharm. Weekbl.* **48**, 230; **48**, 257.

1911. Beobachtungen über die Einwirkung von ultravioletten Strahlen auf höhere Pflanzen. *S. B. Akad. Wiss. Wien.* **120**, Abt. I, 1137.

1913. Ist man berechtigt, die mit dem ultravioletten Lichte der Heraeuslampe erzielten photochemischen Ergebnisse auf die bei der Pflanze im Sonnenlichte vor sich gehenden Prozesse ohne weiteres zu übertragen? *Öst. bot. Z.* **63**, 49.

1913. Suikerbepalingen langs biologischen weg. *Hand. XIV ned. nat.-en geneesk. Congr.* p. 303.

1913. Die Assimilierbarkeit der Maltose durch Hefen. *Biochem. Z.* **52**, 486.

1913. Dosage des sucres dans les confitures et dans les substances analogues par voie chimique et par voie biologique. *C.R. XI Int. pharm. Congr.* p. 1032.

1913. De nieuwere onderzoekingen op het gebied der alcoholgisting. *Chem. Weekbl.* **10**, 239.

1914. De ultrafilternatuur van het levend protoplasma. *Chem. Weekbl.* **11**, 574.

1914. Biochemische suikerbepalingen. Dissertation, Delft.

1916. Een en ander over de chemie der hoogere fungi. *Meded. ned. mycol. Ver.* **7**, 28.

1918. De olie-industrie op Java en de volksvoeding. *Kolon. Studiën*, **2**, 285.

1918. Nieuwe onderzoekingen over copra- en klapperolie. *Teysmannia*, **29**, 571.

1919. Bijdrage tot de kennis der chemische samenstelling van de kapokvezel (vruchthaar van *Ceiba pentandra Gärtn.*). *Hand. I ned.-ind. natuurw. Congr.* p. 163.

1920. (With Iso Reksohadiprodjo.) Coprabereiding op Ceylon. *Teysmannia*, **31**, 97.

1920. Overzicht over de klappervezelnijverheid op Ceylon en aan de Malabarkust en vergelijking der daarbij bestaande toestanden met die in Nederlandsch-Indië. *Ind. Mercuur*, **43**, 721.

1920. Het vraagstuk van de invoering van een klappervezelnijverheid in Nederlandsch-Indië. *Ind. Mercuur*, **43**, 773.

1921. Bevordering der inheemsche nijverheid van regeeringswege. *Kolon. Studiën*, **5**, 153.

1921. Kwaliteitsverbetering der Nederlandsch-Indische Copra. *Teysmannia*, **32**, 243.

1921. Chemie in Nederlandsch-Indië. *Chem. Weekbl.* **18**, 551.

1921. Het bestaansrecht der Nederlandsch-Indische olie-industrie. *Kolon. Studiën*, **5**, 215.

1922. *Microbiologie en Industrie.* Delft: J. Waltman Jr.

1922. Kantteekeningen bij de lijst der op Pasteur betrekking hebbende geschriften. *Chem. Weekbl.* **19**, 518.

1922. Pasteur, de grondlegger der algemeene en toegepaste microbiologie. *Chem. Weekbl.* **19**, 519; *Ned. Tijdschr. Geneesk.* **66**, 2457.

1923. (With Iso Reksohadiprodjo.) Klappervezel- en klappergaren-nijverheid. *Meded. kolon. Inst. Amst.* **20**, Handelmus. 5.

1923-24. Afvalwaterreiniging met behulp van 'activated sludge'. *XXXIII Jversl. Tech. Gezelschap*, p. 59.

1924. Eenheid en verscheidenheid in de stofwisseling der microben. *Chem. Weekbl.* **21**, 266.

1924. Notiz über das Vorkommen von Katalase bei Mikroorganismen. *Hoppe-Seyl. Z.* **138**, 100.

1924. (With F. J. G. de Leeuw.) *Acetobacter suboxydans.* Een merkwaardige azijnbacterie. *Tijdschr. vergelijk. Geneesk.* **10**, 170.

1925. (With H. J. L. Donker.) The unity in the chemistry of the fermentative sugar dissimilation processes of microbes. *Proc. Acad. Sci. Amst.* **28**, 297.*

1925. (With H. J. L. Donker.) The formation of acetylmethylcarbinol and 2-3 butylene glycol in the fermentation of sugars by alcohol yeasts and true lactic acid bacteria. *Proc. Acad. Sci. Amst.* **28**, 314.*

1925. (With H. J. L. Donker.) The catalytic transference of hydrogen as the basis of the chemistry of dissimilation processes. *Proc. Acad. Sci. Amst.* **28**, 605.*

1925. (With H. J. L. Donker and F. Visser 't Hooft.) Über die Bildung von Acetylmethylcarbinol und 2,3-Butylenglykol im Stoffwechsel der Hefe. *Biochem. Z.* **161**, 361.

1925. (With F. J. G. de Leeuw.) *Acetobacter suboxydans,* eine merkwürdige Essigbakterie. *Dtsch. Essigindustr.* **29**, 175; **29**, 188; **29**, 196.

1925. (With C. B. van Niel.) Über Spiegelbilder erzeugende Hefearten und die neue Hefegattung *Sporobolomyces. Zbl. Bakt.* Abt. II. **63**, 1.

1926. Über die Nichtexistenz einiger Fermente. *Hoppe-Seyl. Z.* **158**, 111.

1926. (With A. P. Struyk.) Die Rolle der Phosphate bei der Zuckerdissimilation. *Naturwissenschaften*, **14**, 882.

1926. (With A. P. Struyk.) The function of phosphates in the dissimilation of the hexoses. *Proc. Acad. Sci. Amst.* **29**, 322;* *Ann. Brass. Distill.* **25**, 1; **25**, 17. (In French.)

---

* Papers marked thus also appeared in Dutch in *Verslagen van de gewone vergadering der Wis- en natuurkundige afdeeling.* (*Afdeeling natuurkunde*). Konink. Akademie van wetenschappen te Amsterdam.

1926. (With C. B. van Niel.) Über *Bacillus funicularius* n.sp., nebst einigen Bemerkungen über *Gallionella ferruginea* Ehrenberg. *Planta*, **2**, 507.

1926. (With H. J. L. Donker.) Die Einheit in der Biochemie. *Chem. Zelle*, **13**, 134.

1927. (With A. P. Struyk.) The so-called co-enzyme of alcoholic fermentation. *Proc. Acad. Sci. Amst.* **30**, 569.*

1927. (With A. P. Struyk.) The first phases of the chemistry of the dissimilation of the hexoses. *Proc. Acad. Sci. Amst.* **30**, 871.*

1927. (With A. P. Struyk.) Die Existenz der zellfreien Gärung. *Hoppe-Seyl. Z.* **170**, 110.

1927. De nieuwe voorzitter van het Algemeen Landbouwsyndicaat. *Bergcultures*, **2**, 57.

1927. (With C. B. van Niel.) *Sporobolomyces*—ein Basidiomyzet? *Ann. Mycol., Berl.* **25**, 389.

1927. Toespraak gehouden ter gelegenheid van het gouden doctoraat van Prof. Dr M. W. Beijerinck. *Chem. Weekbl.* **24**, 333.

1928. (With A. P. Struyk.) The first phases of the chemistry of the dissimilation of the hexoses (2nd Part). *Proc. Acad. Sci. Amst.* **31**, 882.*

1928. (With A. P. Struyk.) Über das sogenannte Coenzym der alkoholischen Gärung. Ein Versuch zu einer synthetischen Betrachtung des Coenzymproblems auf experimenteller Grundlage. *Biochem. Z.* **201**, 212.

1928-29. Sur une nouvelle théorie du mécanisme de la fermentation alcoolique. *Ann. Brass. Distill.* **27**, 113, 1928; *Bull. III Ass. Ec. Brass. Louvain*, **29**, 1, 1929.

1929. Note sur le méchanisme de la fermentation alcoolique. *Bull. Soc. Chim. Fr.* (4) **45**, 311.

1929. Over slijmvorming in melk. *Ned. Tijdschr. Hyg.* **3**, 301.

1929. Over bacteriën, welke indol tot indigo oxydeeren. *Ned. Tijdschr. Hyg.* **3**, 308.

1929. (With C. B. van Niel and H. G. Derx.) Über das Butteraroma. *Biochem. Z.* **210**, 234.

1929. (With C. B. van Niel and H. G. Derx.) De bacteriën der roomverzuring en het boteraroma. *Versl. gewone Vergad. Akad. Amst.* **38**, 61.

1930. Atmung, Gärung und Synthese in ihrer gegenseitigen Abhängigkeit. *Arch. Mikrobiol.* **1**, 181.

1931. De grondslagen der biochemie. *Werk. Genoot. Nat.-, Genees.- en Heelk.* (2) **13**, 221.

1931. (With W. J. Hoppenbrouwers.) Ein merkwürdiges Gärungsbakterium: Lindner's *Termobacterium mobile. Arch. Mikrobiol.* **2**, 245.

1931. In memoriam Prof. Dr M. W. Beijerinck. *Ned. Tijdschr. Hyg.* **5**, 173.

1931. *The chemical activities of micro-organisms.* London: University of London Press Ltd.

1931. Einige Bemerkungen über die Einteilung der Bakterien der Gattung *Betacoccus* (Orla-Jensen). In *Festskrift Til Aere for Prof. Dr S. Orla-Jensen.* Odense: Det Danske Syrevaekker-Laboratorium. p. 46.

1931-32. Quelques remarques sur la classification des levures. *Ann. Zymol.* (2) **1**, 48, 1931; *Ann. Brass. Distill.* **30**, 230, 1932; **30**, 241, 1932.

1932. (With W. J. Hoppenbrouwers.) Een merkwaardige gistingsbacterie: Lindner's *Termobacterium mobile. Ned. Tijdschr. Hyg.* **6**, 54.

1932. (With J. K. Baars.) On some physiological artefacts. *Proc. Acad. Sci. Amst.* **35**, 370.

1932. (With A. A. Stheeman.) Zur Nomenklatur der an der alkoholischen Gärung beteiligten Katalysatoren. *Biochem. Z.* **244**, 366.

1932. (With F. L. W. van Roosmalen.) Über die Vergärung der Trehalose. *Biochem. Z.* **245**, 13.

1932. Microbial metabolism and its bearing on the cancer problem. *Science*, **76**, 527.

1932. G. van Iterson Jr., een kwart eeuw hoogleeraar. *Chem. Weekbl.* **29**, 542; *Bergcultures*, **6**, 1226.

1933. (With L. H. C. Perquin.) Zur Methodik der Schimmelstoffwechseluntersuchung. *Biochem. Z.* **266**, 68.

1933. (With L. H. C. Perquin.) Über die Bedingungen der Kojisäurebildung durch *Aspergillus flavus* Link. *Biochem. Z.* **266**, 82.

1933. Some remarks on the metabolism of the lactic acid bacteria and on their rôle in butter making. *J. Soc. Chem. Ind., Lond.* **52**, 367T.

1933. (With W. J. van Reenen.) Über *Azotobacter agilis* Beijerinck. *Arch. Mikrobiol.* **4**, 280.

1933. (With J. C. Hoogerheide.) On the influence of mono iodo acetic acid on the respiration and the fermentation of yeast. *Proc. Acad. Sci. Amst.* **36**, 596.

1933. (With J. C. Hoogerheide.) On the presumed suitability of maltose as a respiration substrate for non-maltose fermenting yeasts. *Proc. Acad. Sci. Amst.* **36**, 605.

1933. (With J. C. Hoogerheide.) L'influence de l'oxygène sur la fermentation alcoolique. *C. R. Acad. Sci., Paris*, **196**, 443.

1933. Mikrobenstoffwechsel und allgemeine Biologie. *Angew. Chem.* **46**, 716.

1934. (With B. Elema and J. W. van Dalfsen.) Über die Beziehungen zwischen den Stoffwechselvorgängen der Mikroorganismen und dem Oxydoreduktionspotential im Medium. I. Mitteilung: Versuche mit denitrifizierenden Bakterien. *Biochem. Z.* **270**, 317.

1934. (With J. C. Hoogerheide.) Über die Beziehungen zwischen den Stoffwechselvorgängen der Mikroorganismen und dem Oxydoreduktionspotential im Medium. II. Mitteilung: Versuche mit Hefearten. *Biochem. Z.* **272**, 197.

1934. Nieuwe onderzoekingen aangaande het chemisme der ademhaling. *Chem. Weekbl.* **31**, 295.

1935. Die bakteriellen Zuckervergärungen. *Ergebn. Enzymforsch.* **4**, 230.

1935. Lactosevergistende gistsoorten. *Chem. Weekbl.* **32**, 568.

1935. De scheikunde van het leven. *Natuurk. Voord. 'Diligentia', Nieuwe Reeks*, **13**, 49.

1935. Die gegenseitigen Beziehungen zwischen dem Oxydationszustande in den Zellen und den Stoffwechselvorgängen. *Proc. VI int. bot. Congr.* **2**, 273.

1936. Bacterial metabolism. *Ann. Rev. Biochem.* **5**, 539.

1936. (With M. T. van den Bout.) Notiz über *Azotobacter agilis* Beijerinck. *Arch. Mikrobiol.* **7**, 261.

1936. De wereld der microben: verleden, heden en toekomst. *Leeuwenhoek ned. Tijdschr.* **3**, 110.

1936. (With C. B. van Niel.) Prospects for a natural system of classification of bacteria. *Zbl. Bakt.* Abt. II. **94**, 369.

1936. (With J. C. Hoogerheide.) Some remarks on the reduction intensity of living cells. *Proc. Acad. Sci. Amst.* **39**, 298.

1936. (With J. C. Hoogerheide.) Beziehungen zwischen den Stoffwechselvorgängen von Hefen und Milchsäurebakterien und dem Redox-Potential im Medium. *Enzymologia*, **1**, 1.

1937. (With Ch. Schnellen.) Über die Vergärung von Rhamnose. *Enzymologia*, **4**, 7.

1937. 's Levens nevels. *Hand. XXVI ned. nat.- en geneesk. Congr.* p. 82.

1937. Beijerinck en de isolatie van *Bacterium radicicola. Natuurwet. Tijdschr.* **19**, 37.

1938. (With A. G. J. Boezaardt.) On the oxidation of glucose by *Acetobacter suboxydans*. *Rec. Trav. chim. Pays-Bas*, **57**, 609.

1939. (With E. L. Molt.) On the production of acetyl-methylcarbinol by *Bacterium coli*. *Proc. Acad. Sci. Amst.* **42**, 118.

1939. (With Anna D. Orla-Jensen.) Notiz über den Erreger der Warmwasserflachsröste in Holland. *Zbl. Bakt.* Abt. II. **101**, 257.

1939. Die Kohlensäure im Stoffwechsel der Lebewesen. *Acta chem. fenn.* **12**, 81.

1939. (With T. Hof.) Een nieuw type bacteriënkleurstof. *Versl. gewone Vergad. Akad. Amst.* **48**, 44.

1939. Over ultramicrobiologie. *Natuurk. Voord. 'Diligentia', Nieuwe Reeks*, **17**, 1.

1939. Het belang van de Landbouwnijverheden en de noodzakelijkheid van een hooger onderwijs dat er verband mede houdt. In *Landbouwinstituut der Universiteit te Leuven. Inwijding van de nieuwe gebouwen.* Leuven: Ceuterick. p. 34.

1939. (With T. Hof and A. G. J. Boezaardt.) On the pigment of *Pseudomonas Beijerinckii* Hof. *Enzymologia*, **7**, 257.

1939. (With A. G. J. Boezaardt.) Note on the biochemical preparation of inosose. *Rec. Trav. chim. Pays-Bas*, **58**, 956.

1939. Microbial metabolism and its significance to the microbiologist. *Proc. III Congr. int. Microbiol.* p. 73.

1939. (With T. Hof.) A new type of bacterial pigment. *Proc. III Congr. int. Microbiol.* p. 227.

1940. Beijerinck, the microbiologist. In *Martinus Willem Beijerinck. His life and his work.* The Hague: Martinus Nijhoff. p. 97.

1940. (With M. Th. J. Custers.) The suitability of disaccharides as respiration and assimilation substrates for yeasts which do not ferment these sugars. *Leeuwenhoek ned. Tijdschr.* **6**, 121.

1941. De microbiologische grondslagen der voedselconservering. *Pharm. Weekbl.* **78**, 778; *Chem. Weekbl.* **38**, 383.

1942. (With G. J. M. van der Kerk and A. van der Burg.) The effect of radiation on light emission by luminous bacteria. I. *Proc. Acad. Sci. Amst.* **45**, 886.

1942. (With G. J. M. van der Kerk and A. van der Burg.) The effect of radiation on light emission by luminous bacteria. II. *Proc. Acad. Sci. Amst.* **45**, 962.

1942. (With G. J. M. van der Kerk and A. van der Burg.) Het bioluminescentieproces der lichtbacteriën. *Versl. gewone Vergad. Akad. Amst.* **51**, 50.

1942. (With E. C. Wassink.) De stofwisseling van de plantaardige cel. In *Leerboek der Algemene Plantkunde, deel II.* Ed. V. J. Koningsberger. Amsterdam: Scheltema and Holkema. p. 198.

1942. (With A. Manten.) Some observations on the metabolism of bacteria oxidizing molecular hydrogen. *Leeuwenhoek ned. Tijdschr.* **8**, 71.

1946. Les Fermentations. *Congrès des sciences pastoriennes pour le cinquantenaire de la mort de Louis Pasteur.* p. 94.

1946-47. Three decades progress in microbiology. *Nat. Verd., Kbh.* **30**, 161, 1946; *Leeuwenhoek ned. Tijdschr.* **13**, 1, 1947.

1947. Recent developments of our insight into fermentation. *Conf. et Commun. Congr. int. Industr. Fermentation.* p. 13.

1947. De toepassing van isotopen ('tracers') in de biologie. *Hand. XXX ned. nat.-en geneesk. Congr.* p. 95.

1947. Enige belangwekkende uitkomsten verkregen bij de toepassing van isotopen in het stofwisselingsonderzoek. *Chem. Weekbl.* **43**, 164; **43**, 180.

1947. (With Ch. G. T. P. Schnellen.) On the fermentation of carbon monoxide by pure cultures of methane bacteria. *Arch. Biochem.* **14**, 57.

1947. (With Ch. Schnellen.) Studies on methane fermentation. *Proc. IV Congr. int. Microbiol.* p. 541.

1947. Rede ter gelegenheid van het Honderdjarig Jubileum van het Kon. Inst. v. Ingenieurs. *De Ingenieur*, **59**, A, 329.

1949. Krachten der microbenwereld. *Jaarb. Tech. Hogesch. Delft*, p. 5.

1949. Homo militans. *Haarlemse Voordrachten*, **8**, 1.

1949. Microbiologie. In *Eerste Nederlandse Systematisch Ingerichte Encyclopaedie, deel VI.* Amsterdam. p. 18.

1950. (With J. Visser.) The determination of micro-organisms in air. *Leeuwenhoek ned. Tijdschr.* **16**, 299.

1950. (With J. Visser.) Some observations on air filtration. *Leeuwenhoek ned. Tijdschr.* **16**, 311.

1951. (With J. De Ley and A. Rijven.) The formation and consumption of lactobionic and maltobionic acids by *Pseudomonas* species. *Leeuwenhoek ned. Tijdschr.* **17**, 1.

1951. (With J. C. M. van Zijp.) The production of homogentisic acid out of phenylacetic acid by *Aspergillus niger*. *Leeuwenhoek ned. Tijdschr.* **17**, 315.

1951. Inleiding tot de biokatalyse en zijn problemen. *Chem. Weekbl.* **47**, 925.

1951-52. Rede, gehouden als voorzitter van de Afd. voor de Wis- en Natuurkundige Wetenschappen, in haar vergadering van 27 October 1951 naar aanleiding der oprichting van de Kon. Akad. v. Wetenschap op 26 October 1851. *Jaarb. Akad. Amst.* p. 80.

1952. Microbial metabolism and its industrial implications. *Chem. & Ind.* p. 136.

1952. In memoriam Hendrik Christoffel Jacobsen. Obituary Notice. *Leeuwenhoek ned. Tijdschr.* **18**, 167.

1952-53. Levensbericht van Sergei Nikolaevitch Winogradsky. *Jaarb. Akad. Amst.* p. 315.

1953. The changing appraisal of the microbe. Leeuwenhoek lecture. *Proc. Roy. Soc.* B, **141**, 147.

1953. Rede Lorentz-herdenking. *De Gids*, **116**, 160.

1953. An aspect of the promotion of science. *News Report, Nat. Acad. Sci. Nat. Res. Council*, **3**, 33.

1953. Some aspects of nitrate reduction. *Proc. VI Congr. int. Microbiol., Symp. Metabolismo Microbiol.* p. 71.

1953. (With J. P. VAN DER WALT and A. J. VAN TRIET.) Pulcherrimin, the pigment of *Candida pulcherrima*. *Proc. Nat. Acad. Sci. Wash.* **39**, 583.

1954. Toespraak. In *Nicolaas Beets en Rembert Dodoenshulde*. Brussels: Koninklijke Vlaamse Academie van Wetenschappen, Letteren en Schone Kunsten van België. p. 70.

1954. (With E. H. HOUWINK.) An attempt to produce more uniformly vulcanizing natural rubber by a bacterial treatment of latex. *Arch. Rubber Cultiv.* **31**, 277.

1954. (With W. VERHOEVEN.) Studies on true dissimilatory nitrate reduction II. The mechanism of denitrification. *Leeuwenhoek ned. Tijdschr.* **20**, 241.

1954. (With W. VERHOEVEN.) Studies on true dissimilatory nitrate reduction IV. On adaptation in *Micrococcus denitrificans*. *Leeuwenhoek ned. Tijdschr.* **20**, 337.

1954. A tribute to Rutgers University and the state of New Jersey. *Sci. Mon., N.Y.* **79**, 359.

1954-55. Microbe en Leven. *Jaarb. Akad. Amst.* p. 225.

1954-55. Levensbericht van Sir Alexander Fleming. *Jaarb. Akad. Amst.* p. 262.

1955. From Dutch Settlements to the Rutgers Institute of Microbiology. In *Perspectives and horizons in microbiology*. A symposium edited by Selman A. Waksman. New Brunswick, N.J.: Rutgers University Press. p. 213.

1955. (With J. H. BECKING.) Some observations on the nitrogen fixing bacteria of the genus *Beijerinckia*. *Ann. Acad. Sci. fenn.* ser. A, sec. II, **60**, 367.

1956. (With C. B. VAN NIEL.) *The microbe's contribution to biology*. Cambridge (Mass.): Harvard University Press.

1956. *Pseudomonas aureofaciens* nov. spec. and its pigments. *Bact. Proc.* p. 33.

1956. *Pseudomonas aureofaciens* nov. spec. and its pigments. *J. Bact.* **72**, 406.

Section B

Dissertations of Kluyver's students

1923. C. A. H. VON WOLZOGEN KÜHR. Onderzoekingen aangaande de mikroflora aanwezig in normaal en serehziek suikerriet. Delft.

1925. F. VISSER 'T HOOFT. Biochemische onderzoekingen over het geslacht *Acetobacter*. Delft.

1926. H. J. L. DONKER. Bijdrage tot de kennis der boterzuur-, butylalcohol- en acetongistingen. Delft.

1926. L. E. DEN DOOREN DE JONG. Bijdrage tot de kennis van het mineralisatieproces. Delft.

1928. H. R. BRAAK. Onderzoekingen over vergisting van glycerine. Groningen.

1928. A. P. STRUYK. Onderzoekingen over de alcoholische gisting. Delft.

1928. C. B. VAN NIEL. The propionic acid bacteria. Delft.

1928. M. A. SCHEFFER. De suikervergisting door bacteriën der coligroep. Delft.

1929. K. HOLWERDA. Over de contrôle en de mate van betrouwbaarheid van het chloreringsproces voor drinkwater, speciaal voor de tropen. Delft.

1930. J. B. VAN DER LEK. Onderzoekingen over de butyl-alkoholgisting. Delft.

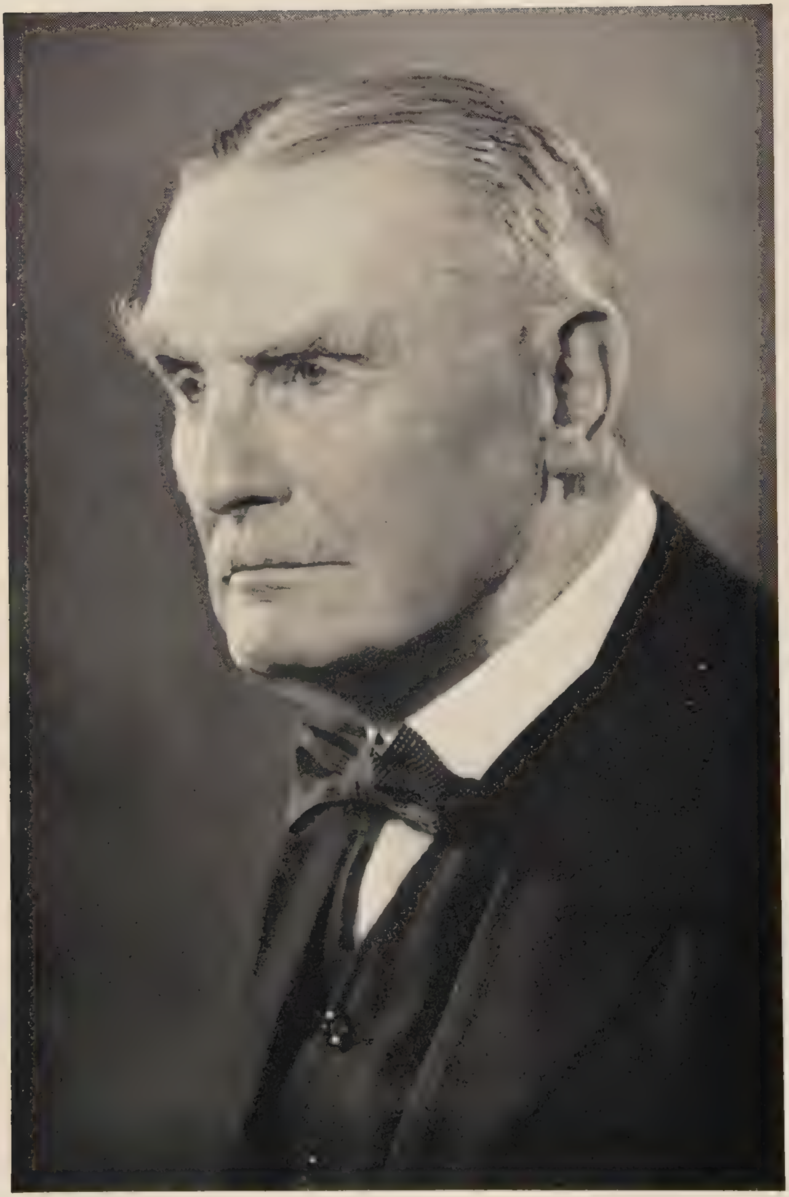
1930. J. K. BAARS. Over sulfaatreductie door bacteriën. Delft.

1931. N. M. STELLING-DEKKER. Beiträge zu einer Monographie der Hefearten. I. Teil. Die sporogenen Hefen. Utrecht.
1932. B. ELEMA. De bepaling van de oxydatie-reductiepotentiaal in bacteriëncultures en hare beteekenis voor de stofwisseling. Delft.
1933. F. M. MULLER. On the metabolism of the purple sulphur bacteria in organic media. Utrecht.
1934. T. Y. KINGMA BOLTJES. Onderzoekingen over nitrificeerende bacteriën. Delft.
1934. J. LODDER. Beiträge zu einer Monographie der Hefearten. II. Teil. Die anaskosporogenen Hefen. Erste Hälfte. Utrecht.
1934. R. H. MEES. Onderzoekingen over de biersarcina. Delft.
1934. J. VAN DEN BERGE. Beoordeeling van de waarde van fungicide stoffen voor houtconserveering. Delft.
1934. P. A. ROELOFSEN. On photosynthesis of the *Thiorhodaceae*. Utrecht.
1935. T. HOF. Investigations concerning bacterial life in strong brines. Leiden.
1935. J. C. HOOGERHEIDE. Bijdrage tot de kennis van de reactie van Pasteur. Delft.
1936. G. GIESBERGER. Beiträge zur Kenntnis der Gattung *Spirillum* Ehgb. Mit besonderer Berücksichtigung der Atmungsprozesse bei den Vertretern dieser Gattung. Utrecht.
1938. H. D. MAYER. Das "Tibi"-Konsortium nebst einem Beitrag zur Kenntnis der Bakterien-Dissoziation. Utrecht.
1938. K. L. VAN SCHOUWENBURG. On respiration and light emission in luminous bacteria. Delft.
1938. L. H. C. PERQUIN. Bijdrage tot de kennis der oxydatieve dissimilatie van *Aspergillus niger* van Tieghem. Delft.
1940. M. TH. J. CUSTERS. Onderzoekingen over het gistgeslacht *Brettanomyces*. Delft.
1941. F. A. M. J. SMITS VAN WAESBERGHE. Onderzoekingen over microben-amylasen. Delft.
1942. P. B. ROTTIER. Fluorometrische en spectrophotometrische bepaling van lactoflavine in micro-organismen. Delft.
1947. CH. G. T. P. SCHNELLEN. Onderzoekingen over de methaangisting. Delft.
1948. A. KAARS SIJPESTEIJN. Cellulose-decomposing bacteria from the rumen of cattle. Leiden.
1952. W. VERHOEVEN. Aerobic sporeforming nitrate reducing bacteria. Delft.
1952. J. P. VAN DER WALT. On the yeast *Candida pulcherrima* and its pigment. Delft.

Section C

References

Kingma Boltjes, T. Y. (1939) A. J. Kluyver als leermeester. *Chem. Weekbl.* **36**, 319.
——— and Verhoeven, W. (1956) In memoriam Prof. Dr Ir A. J. Kluyver. *Chem. Weekbl.* **52**, 661.
——— and Wassink, E. C. (1957) A. J. Kluyver (1888-1956). *Enzymologia*, **18**, 3.
Perquin, L. H. C. (1939) Bibliographie 1911-1939 van de publicaties van Prof. Dr Ir A. J. Kluyver. *Chem. Weekbl.* **36**, 320.
Van Iterson, G. Jr. (1939) A. J. Kluyver als leerling, als jeugdig bioloog en als collega. *Chem. Weekbl.* **36**, 307.
Van Niel, C. B. (1939) A. J. Kluyver als mikrobioloog en als biochemikus. *Chem. Weekbl.* **36**, 310.
——— (1949) The "Delft School" and the rise of general microbiology. *Bact. Rev.* **13**, 161.
——— (1957) Albert Jan Kluyver, 1888-1956. *J. Gen. Microbiol.* **16**, 499.



GERALD PONSONBY LENOX-CONYNGHAM

1866-1956

Gerald Ponsonby Lenox-Conyngham was born on 21 August 1866 at Springhill, Moneymore in County Londonderry, Ireland. He was the seventh of the ten children of Sir William Fitzwilliam Lenox-Conyngham, K.C.B., D.L., J.P. His father's family, the Conyngham's, had come to Ireland, probably from Ayrshire, early in the seventeenth century. His great-great-grandmother inherited the family house and married Clotworthy Lenox who added his wife's family name to his own.

His mother, Laura Calvert Arbuthnot, was the daughter of George Arbuthnot of Ockley in Surrey who spent many years in India and founded the once flourishing firm of Arbuthnot & Co. of Madras. An account of this family has been given by Mrs P. S.-M. Arbuthnot in a book entitled *Memoirs of the Arbuthnots of Kincardinshire and Aberdeenshire.*

When the children reached school age the family moved to Edinburgh in order that the boys might attend the excellent schools available there. Lenox-Conyngham was educated at the Edinburgh Academy from the age of nine till he was sixteen. While there he decided to try to join the Royal Engineers and, after a year of private coaching, took the Woolwich entrance examination soon after his seventeenth birthday. He passed first and in later life ascribed this success to the care with which his aunt, Miss Eleanor Arbuthnot had selected his teachers.

In 1885, at the age of nineteen, he passed first out of Woolwich, being nearly 2000 marks above the man below him, and receiving the Sword of Honour, the Pollock Medal and other prizes. His instructor in mathematics was Captain P. A. MacMahon, later a Fellow of the Royal Society.

Now a lieutenant in the Royal Engineers, Lenox-Conyngham spent two years at the School of Military Engineering at Chatham. Here he applied to be sent to India, sailed at the end of 1887 and arrived in Bombay a month later. He was first posted to Rawalpindi and was principally occupied in a scheme for its defence in the event of an attack from the north-west.

In 1889 he volunteered for transfer from the Military Works Service to the Survey of India and was directed to join the Trigonometrical Branch at its summer headquarters at Mussoorie. Thus, at the age of twenty-three, he entered on a career as a surveyor and a geodesist which was to be the main occupation and the main scientific interest of his long life.

When Lenox-Conyngham joined it, the Great Trigonometrical Survey of India had been in existence for 89 years. Col. H. R. Thuillier was Surveyor

General and the organization was divided into a Topographical Branch, which did the detailed surveying and produced maps, and a Trigonometrical Branch which was concerned with the framework of the survey and its various scientific offshoots, including triangulation, levelling, astronomical observations for latitude, longitude and azimuth, gravity measurement and the measurement and analysis of the tides. For all but one year of his career in the survey Lenox-Conyngham was a member of the Trigonometrical Branch. The survey had carried through the immense task of observing and reducing a system of triangulation covering the whole of India. The results of this, and the associated work of levelling and astronomical observation, were being published in the long series of large, black, quarto volumes which may well prove to be one of the most permanent memorials of British rule in India.

The observations included an extensive series of measurements of longitude along parallels of latitude intended to determine the curvature of the geoid in a direction perpendicular to that given by the older observations of latitude. These longitudes had been determined with great care with the aid of the electric telegraph, but showed puzzling discrepancies. In 1889 Captain S. G. Burrard was about to take up these observations again with the object of extending them and trying to find the cause of the discrepancies. Lenox-Conyngham became his assistant, and in October, 1889, the two set out with two transit instruments to determine a series of differences of longitude. In his obituary notice of Burrard, Lenox-Conyngham has given a graphic description of how, at a meeting in Agra, to determine their relative personal equation, Burrard suggested the cause of the trouble. A detailed account of the matter has been given by General J. T. Walker (*Phil. Trans.* A, **186**, 1895) and in Vol. 15 of the *Account of the operations of the Great Trigonometrical Survey of India;* it is sufficient here to say that the trouble lay in the collimator not illuminating the whole of the telescope objective. Imperfections in the figure of the lenses or errors in focusing then introduce an error in the determination of the line of collimation and a discrepancy on reversing the telescope. Reversing the telescope was a considerable operation* and was only done three times in six nights' observations; the error was, therefore, not removed from each night's observations by reversal. For some now unaccountable reason the observations had been reduced in such a way that those for the two positions of the telescope had not equal weight and some part of the collimation error had, therefore, appeared in the final longitudes. Experiments and the recomputation of the older observations during the summer of 1890 left no doubt of the correctness of the explanation, and showed that differences of longitude could be satisfactorily measured with portable instruments.

The collaboration with Burrard led to a friendship that lasted till he died in 1943 and was one of the main influences in the development of Lenox-Conyngham's scientific interests. He wrote in 1944: 'That partnership and friendship I look back upon with a satisfaction and gratitude that cannot be adequately expressed in words. It was an inestimable privilege to be in such constant and close touch with Burrard's clear mind, his penetrating intelligence and, above all perhaps, with the delightful humour of his conversation. I have never known any one whose talk was so interesting or of whom it could be said with more truth that every word he spoke came fresh-minted from his mind. To work with him was an unending source of pleasure and refreshment.'

---

* Lenox-Conyngham wrote 'The older instruments which had been used hitherto could only be reversed by being lifted on a sort of cradle by four men, carried outside the observatory tent and, after a kind of countermarch, brought in again and replaced on the supports with the east pivot where the west had been. With well drilled men the change was made very smoothly and without jar or shake.'

In November 1890 Lenox-Conyngham married Elsie Margaret, the eldest daughter of Surgeon General Sir A. Frederick Bradshaw, K.C.B., K.H.P., who later became head of the Army Medical Service in India. They had one daughter, Enid. Both were twenty-four years of age when they married, and both attained the age of ninety. In her younger days Lady Lenox-Conyngham accompanied her husband on his field trips, and in this way they visited many out-of-the-way parts of India. Many friends remember with gratitude the kindly hospitality that he and Lady Lenox-Conyngham dispensed for so many years both in India and at Cambridge.

The years 1891-1894 were spent in observations of latitude and longitude at many places in all parts of India. The Talcott method of finding latitude was here employed in India for the first time.

In 1894 Burrard and Lenox-Conyngham undertook a redetermination of the longitude of India relative to Greenwich. The value then in use was an old one based on observations made in 1815 before the telegraph was available. Although a connexion existed via the submarine cable to Aden and Suez it was decided to make a new start using the land line across Europe and Persia. In those days the Government of India owned and worked the submarine cables connecting Karachi with Bushire and also the land line from Bushire to Teheran, and the Indo-European Telegraph Company of London owned and worked a line from England across Germany and Russia to the Caucasus and thence to Teheran. Two new reversible transit instruments were obtained and the connexion from Karachi to Bushire was made by the spring of 1895. Burrard and Lenox-Conyngham then left for England. The first link observed from that end was a comparison of Greenwich and Potsdam with interchange of observers. It was decided to compare Potsdam and Teheran in one step, the signal being reshaped by a relay at Odessa. Lenox-Conyngham, therefore, went to Teheran and observed there, while Burrard remained in Potsdam. The observations required clear weather at Teheran and Potsdam and an intact telegraph line between them. The work was much interrupted by cloudy weather and by damage by snow storms to the telegraph line across the Caucasus. When the work at Potsdam was complete Burrard moved to Bushire while Lenox-Conyngham remained in Teheran until the Teheran-Bushire connexion was made. The results were most satisfactory, a redetermination thirty years later using wireless signals giving a longitude differing by only 0·02 sec from that found by Burrard and Lenox-Conyngham.

On the conclusion of the longitude observations Lenox-Conyngham took leave in England, returning to India in 1897 to take charge of the computing office. In January 1898 a total eclipse of the sun was due to occur in northern India and Burrard and Lenox-Conyngham were given charge of two camps provided by the Government of India. Lenox-Conyngham's camp was at Pulgaon, about fifty miles south-west of Nagpur, its domestic arrangements were seen to by Mrs Lenox-Conyngham. The astronomers who came to this camp were H. F. Newall and Major E. H. Hills, both accompanied by their wives. The weather was fine and the observations were successfully carried through. This meeting with the Newall's led to a friendship between the two families which ripened into what Lenox-Conyngham has called 'one of our most cherished possessions'. Newall kept in touch with Lenox-Conyngham's subsequent work and probably had much influence on his appointment to a post at Cambridge twenty years later.

During the winter of 1898-1899 Lenox-Conyngham was engaged in making observations of latitude and azimuth at points in the neighbourhood of Kalianpur. This was the last occasion on which the 24 in. theodolite with five microscopes and Colonel Strang's very large zenith sector were used. Kalianpur was the origin of the Indian triangulation and it was thought that it would be better to base the direction of the vertical there on the mean of observations at a group of nearby stations rather than entirely on observations at the origin itself. This would be the better course if the deflexions of the vertical were substantially uncorrelated over the distances that separated the origin from the subsidiary stations. This was at that time believed to be so, the belief having arisen from the discussion by Archdeacon Pratt of Calcutta of the latitude observations of Everest and Waugh. Pratt had shown that the attraction of even so great a feature as the Himalayas fell off rapidly to the southward and suggested that this was due to a 'root' of relatively light material beneath the mountains. If the effect of so great a feature was negligible at a distance of a few hundred miles it was natural to suppose that lesser features would have lesser and more local effects and that deflexions of the vertical could be treated as independent random errors. In fact the observations showed that the relative deflexions of the vertical between Kalianpur and the surrounding stations were small.

At about this time Burrard began a scrutiny of the substantial collection of observations of latitude acquired by the Survey over nearly a century. He concluded that the deflexions in north India were far from uncorrelated and were arranged systematically in zones parallel to the mountains. He ascribed them to the attraction of a 'hidden range' to the south of the Gangetic plain.

The most direct method of locating such a hidden mass is by the measurement of the acceleration due to gravity. It was, therefore, decided to take up again the pendulum observations that had been abandoned after the death of Basevi in 1871, while working with the Royal Society's pendulums at a height of over 15000 ft. in the Himalayas. Like most measurements of gravity made during the nineteenth century, those of Basevi and Heaviside were subject to large errors from the effects of the sway of the support on which they were swung. Methods for measuring this correction had been devised by R. Schumann of the Prussian Geodetic Institute at Potsdam and it was decided to seek the advice of F. R. Helmert, the director of that Institute. Helmert recommended the purchase of an apparatus with four half-second pendulums designed by von Sterneck of Vienna, and offered to have it modified at Potsdam and to determine the constants of the pendulums.

Burrard and Lenox-Conyngham visited Potsdam while on leave in 1902. They had met Helmert and his staff during the longitude observations of 1895 and were well received and instructed in the use of the equipment. This visit made a considerable impression on Lenox-Conyngham, and led him thirty years later to arrange for the present writer to visit Potsdam very shortly after becoming his assistant.

On returning to England the pressure and temperature coefficients of the pendulums were redetermined at Kew Observatory with the assistance of the then recently formed National Physical Laboratory. The results agreed well with those obtained at Potsdam. Observations were then made at Kew and at Greenwich partly to connect the two observatories and partly to provide a base for the Indian observations. In the published report on the Indian observations these measurements in England are discussed in considerable detail, the densities of the rocks being considered in an appendix by A. Strahan. This appendix contains a discussion of the form of the Palaeozoic floor of south-east England which was of great use when the study of the floor was taken up in Lenox-Conyngham's Cambridge department in the 1930s.

From 1903 to 1908, that is from the age of thirty-seven to forty-two, Lenox-Conyngham was engaged in the series of gravity measurements in India which is perhaps his most important contribution to science. The pendulums were timed by a clock which was rated by astronomical observations with a transit instrument, and no precaution was omitted that could improve the accuracy of the results. Year after year Lenox-Conyngham and his cavalcade of over a hundred assistants, servants and porters wended their way along lines running from the plains high up into the Himalayas and back again. They obtained thirty-nine values of the gravitational acceleration including one at a height of nearly 12000 ft. at Sandakphu near Darjeeling.

The results are described in great detail in *Professional Paper* No. 10 of the Survey of India. A small excess of gravity was found over Burrard's 'hidden range' and a much larger deficit on the plain nearer the mountains. The discussion of the results is brief and does not attempt to relate them to the structure and history of the region. No mention is made of the papers of C. S. Middlemiss of the Geological Survey of India who had regarded the Gangetic plain as marking the site of a 'foredeep' of the Himalayas. The geologists for the most part seem either to have ignored, or to have misunderstood, the results, though the omniscient Suess mentions them in his discussion of the foredeep of the Himalayas in Vol. 4 of *The face of the earth*, published a few years later. The failure of collaboration between the geologists and geodesists in India was an unfortunate feature in the development of the earth sciences all over the world and one whose effects are still felt. I have the impression that Lenox-Conyngham did not wish, at that time, to say that Burrard's 'hidden range' was not the main feature and that both the deflexions of the vertical and the gravity anomalies were due mainly to a great thickness of light sediments further north. Later both he and Burrard made this quite clear; for example, in 1931 he wrote 'The pendulums do not reveal any great excess of mass where the hidden range was supposed to be, but they show that there is a great defect of mass all along the foot of the Himalayas and for some distance from them.' This strip of negative gravity anomalies and deficient density is, I believe, the first example of a phenomena which Vening Meinesz and others have subsequently shown to be of widespread occurrence particularly on the outer edges of island arcs. These strips form part of a complex of topographic, seismic and volcanic phenomena which is probably associated with mountain building. The one found by Lenox-Conyngham is a sediment-filled trough in front of the Himalayas, but just how such troughs are formed and what is their relation to the deeper structure is still a matter of controversy.

The work of Burrard and Lenox-Conyngham on the deflexions of the vertical and the gravity anomalies in India revived interest in this branch of geodesy, by showing that its results have a wider interest than the mere study and reduction of errors in surveying. In particular Hayford has acknowledged that it was largely the Indian work that stimulated his investigations in the U.S.A.

In 1908 Lenox-Conyngham took leave in England, during which he visited Paris with Sir David Gill in connexion with the provision of a new apparatus for the comparison of standards of length. Later he wrote two papers on the use of invar wires in the measurement of base lines.

On returning to India he spent a year at the headquarters office of the Survey of India in Calcutta where he had charge of map printing and instrument making. He was then transferred to Dehra Dun and took charge of precise levelling. In 1912 he took leave in England and on return was promoted to be Superintendent of the Trigonometrical Survey, Burrard being Surveyor General. In 1914 he was promoted to Colonel. In the normal course of events Lenox-Conyngham could have expected to become Surveyor General in 1915, but, after the outbreak of war, it was decided that Burrard's term should be extended. Lenox-Conyngham, therefore, remained Superintendent until he left India in 1920. The activities of his department were reduced to a low level during the war by the withdrawal of the younger officers to France or Mesopotamia. He visited Bagdad in 1918 to plan the levelling operations which were required by a scheme for the control of the Tigris and Euphrates. Just before leaving for Bagdad he heard that he had been elected a Fellow of the Royal Society; he served on the Council during 1921-1922 and 1934-1936. In 1919 he was made a Knight Bachelor.

On returning to England he decided to settle in Oxford. He had only been there a few months when he was asked to join a committee which was to consider the promotion of the study of Geodesy in Cambridge. Soon after the war it had been suggested that a National Geodetic Institute might be set up. When this came to nothing it was hoped that something on a more modest scale might be done in Cambridge. Unfortunately, the University could provide no funds for the purpose, and this scheme, too, would have come to nothing had not Trinity College offered to provide a Praelectorship in Geodesy. They were able to do this as Gowland Hopkins was about to vacate a Praelectorship on being made Professor of Biochemistry. The assistance that Trinity has given in the development of biochemistry and geodesy in Cambridge is a striking example of the advantages of some diversification in the financing of research; it is improbable that the study of either subject would have been started when it was if the University had been the sole channel for funds.

The Praelectorship was offered to Lenox-Conyngham. In a speech that he made a few days before he died he described the delight with which he accepted an offer that would transfer him from the rather tedious life of a retired colonel in Oxford to active work in his own subject. He took up residence in Cambridge for the Michaelmas Term of 1921 and was made a Fellow of Trinity. In the next year the University created a Readership in Geodesy for him, but assigned no salary to the post.

The University was now committed to promote teaching and research in geodesy and had been fortunate enough to obtain, at no cost to itself, a man of outstanding experience and ability. Unfortunately, it was able to devote almost no funds to the subject. A small brick building and a wooden observing hut were constructed at the back of the Observatory and some obsolete equipment was given by the Survey of India. With this meagre provision Lenox-Conyngham started to teach geodesy to a small group of undergraduates reading for Part 2 of the Geographical Tripos. He gave these lectures for 26 years; they are continued today by J. E. Jackson. They gave a detailed theoretical and mathematical exposition of triangulation, levelling and geodetic astronomy. The amount of practical work was limited by lack of instruments and the difficulty of fitting it into the undergraduate timetable. The classes were never large, but from 1924 onwards many colonial survey departments sent their officers to Cambridge to take the course and, over a long period of years, a large proportion of those serving in these surveys received a thorough grounding in the theory and practice of their subject.

It is difficult for a man working entirely alone to prosecute experimental research effectively, the more so when he is without equipment or funds. Lenox-Conyngham decided to try to construct a pendulum apparatus which would be an improvement over that used in India. Sir Horace Darwin, who was Chairman of Cambridge Instrument Company gave financial support and assisted in the design and construction of an instrument in which invar pendulums were swung in air at reduced pressure. Vening Meinesz had then recently introduced the practice of swinging two pendulums in opposite phase to avoid sway of the support and the effects of movement of the ground. This principle was adopted in Lenox-Conyngham's instrument. The first instrument had a very solid bronze case and proved almost intolerably heavy. Another was constructed of aluminium alloy and holding two pendulums instead of three. This was entirely successful and is still in use for the most precise long distance gravity connexions. Eight instruments have been made in all and have been used in many parts of the world.

Various difficulties were experienced in getting consistent results and Lenox-Conyngham decided that a radical change was required in the timing arrangements. The direct use of radio time signals naturally suggested itself, but he felt that to develop the necessary equipment he required an assistant who was a physicist. Just at this time, the University had formed the departments studying the earth into a 'Faculty of Geography and Geology'. Lenox-Conyngham's one man 'School of Geodesy' formed part of this faculty and became the 'Department of Geodesy and Geophysics'. Expansion was now possible, and in 1931 Harold Jeffreys was appointed to a readership in geophysics, and a post of demonstrator was instituted to provide an assistant for Lenox-Conyngham. Provision was also made for the appointment of a laboratory assistant; as Lenox-Conyngham was indisposed the selection for this post was made by Lady Lenox-Conyngham who selected L. H. Flavill, then a boy of sixteen. This appointment had a great influence on the success of the department since the new assistant developed into a designer and maker of instruments of exceptional skill and resource, who is still in charge of the Department's workshop. To fill the post of demonstrator Lenox-Conyngham applied to Rutherford, who suggested the author of this notice. Thus by an extraordinary piece of good fortune I met Lenox-Conyngham and secured a job just when the economic depression was at its height and jobs were almost unobtainable. At first I was a little suspicious; I knew nothing of colonels retired from India and nothing of geodesy, but these doubts soon vanished as I got to know him. Looking back I am amazed at the way he encouraged and supported a succession of rather bizarre enterprises.

We started at once on the problem of improving the method of using the pendulum apparatus. A photographic method was developed for comparing the pendulums with wireless signals and every source of uncertainty that either of us could think of was investigated. The equipment remained essentially the same until after the war and a long series of measurements was made in many parts of the world.

In 1935 the island of Montserrat in the West Indies had been suffering severely from earthquakes for two years; the inhabitants, therefore, sent a petition to the Governor of the Leeward Islands asking that the conditions in the island should be scientifically investigated. The Governor sent the petition to the Colonial Office who asked the Royal Society to advise them. The Royal Society sent A. G. MacGrigor and C. F. Powell to investigate the island and asked Lenox-Conyngham to go out a few months later to report on the work. He visited the island for a fortnight in July 1936, by which time the earthquakes had almost ceased.

In 1935 B. C. Browne joined the Department and its work broadened and extended into fields of seismology and geothermal measurement where Lenox-Conyngham had little previous knowledge. He continued to take a close interest, particularly in Browne's work on the measurement of gravity at sea. Here his friendship with Vening Meinesz was of great assistance. He went to great trouble to use his influence and powers of persuasion to raise money and get facilities. I remember in particular the elaborate letters he wrote to landowners asking permission for us to 'make impulses' on their land. These explained in great detail the purpose and nature of the work, but passed rather lightly over the nature of the proposed impulses, which were in fact explosions of up to 15 lb. of gelignite. Nothing seemed seriously to disturb him; when I confessed that a supposed I.R.A. atrocity described in the morning papers was in fact a departmental indiscretion with explosives and that the police were looking for us, his only comment was 'I don't think I should tell them, it will be interesting to see if they can catch you.'

I think it pleased him greatly to see work going on in his department, research students being attracted to it and papers being published, and to know how much we relied on his advice and judgment and on his wide contacts in scientific and official circles. In the eight years we worked together before the war he was indefatigable in borrowing everything we needed from galvanometers to submarines and survey ships. The money came largely from the Royal Society. He always read the manuscripts of papers written by members of the department with great care and his comments on presentation and grammar had a most beneficial influence on the literary style of those working in the department.

In the period between the wars he travelled extensively. He and his daughter attended the Pacific Science Congress in Australia in 1923, visiting the Great Barrier Reef and New Zealand after the Congress. In 1926 they were at the next meeting of the Pacific Science Congress in Tokyo; on the way home he visited Ceylon to enquire whether the Surveyor General considered the training that was being given in Cambridge to the Survey probationers was satisfactory. He attended the meetings of the International Union of Geodesy and Geophysics in Rome in 1922, in Madrid in 1924, in Lisbon in 1933 and in Edinburgh in 1936. He greatly enjoyed these meetings at which he made many friends and had considerable influence, particularly in the Association of Geodesy.

During the 1939-1945 war he continued his lectures, the audience being largely girls from Girton and Newnham. The experimental work of the department ceased as all those engaged in it went to work for the Navy. He became an Air Raid Warden and was appointed Senior Warden of the Sector whose headquarters were in the dining room of his house at 69 Grange Road. The log-book of this post is now in the library of Trinity College.

After the war B. C. Browne, L. H. Flavill and I returned to Cambridge, and, with research students, who included M. N. Hill, started work again where it had been left in 1939. We now had the advantage of the improved techniques developed during the war and rapid progress was made in seismological and gravity work at sea. In all this Lenox-Conyngham took a keen and knowledgeable interest. This interest continued after his retirement and even when he was eighty-nine years of age I received a letter from him asking about the measurement of heat flow at sea, a type of measurement that had only been developed a few years before.

In 1946 a lunch was held in Trinity to celebrate his and Lady Lenox-Conyngham's 80th birthdays. It was attended by about sixty friends. Their health was proposed by the Master, G. M. Trevelyan. In reply Lenox-Conyngham spoke of the many friends he had made in Cambridge and of his debt to them, to Trinity and to the University for enabling him to have a second career in Cambridge.

In the summer of 1947 he retired from his Readership and from the Praelectorship, but retained his Fellowship of Trinity. For the next nine years he lived quietly in his house in Grange Road, entertaining his friends, dining fairly frequently in College, attending scientific meetings that interested him, and occasionally visiting London for gatherings such as the Royal Society Soirée or meetings of the board of a Ceylon tea company of which he was a director.

In October 1956 he and Lady Lenox-Conyngham attended a lunch to celebrate their 90th birthdays, which both greatly enjoyed. I proposed their health; in reply he said how fortunate he considered himself to have been. He recalled the pleasant and varied life that he and his family had lived in India and the many friends they had made there and in Cambridge. He gave an account of the history of the Department of Geodesy and Geophysics and mentioned the satisfaction he had in seeing it established in the house of his old friend, Professor Newall, who had done so much to bring it into being. Not long after this he became unwell and died peacefully after a short illness on 27 October 1956.

Few men can have had so full or so useful a life as Lenox-Conyngham. He had two complete careers, one in India lasting thirty-three years from 1887 to 1920, and the other in Cambridge lasting twenty-six years from 1921 to his retirement in 1947. Both were outstandingly successful. His geodetic work in India combined immediate practical importance for survey with a wider importance for our understanding of the structure of the country. In Cambridge he founded the Department of Geodesy and Geophysics and built it up to be one of the most important centres of geophysical research. He also developed the teaching of surveying during a period when men were needed in increasing numbers by survey services all over the world.

At the party to celebrate his 80th birthday the Master of Trinity said: 'He is a scholar, a soldier and a great public servant, and he looks all three.' He did indeed, he had a commanding presence and as he grew older became even more dignified and more impressive. The photograph reproduced here was taken when he was about eighty-five, a photograph taken about 1931 may be seen in the *Bulletin Geodesique* for 1947. Imposing as he was one could not be with him long without some shrewd or entertaining comment showing that here was not merely a dignified and distinguished man, but a wise and sympathetic friend of great experience and wide and humane interests. He had a solid confidence in his position in the world and in the conventions in which he had been brought up; this was well illustrated by an incident that occurred a few weeks before he died. I was his guest at a feast in Trinity and was wearing a white tie and a white waistcoat. He commented on this and explained that, as there was not to be dancing, I should have worn a black waistcoat with my white tie, he seemed entirely unmoved by the fact that he was the only man present who did so. This rigid attitude to matters of convention was combined with an extraordinary openness of mind in matters of science. When new methods or new work were suggested it was usually easy to get his interest and his support. He was always delighted to see the new developments of physics and engineering applied to the problems he had worked on many years before. He never pretended to understand details of the modern equipment, but he liked to be shown how it worked and to discuss projects for using it. This combination of genuine interest, friendliness and lack of pretence enabled him to be outstandingly successful as the head of a department in which most of the staff were forty years his junior.

In his relations with all kinds of people he was at the same time considerate, firm and kindly. I have heard him congratulate a man who had been made a night watchman in a library, telling him how pleased he was that this great responsibility had fallen to him in exactly the words and tone of voice he would have used if the man had been made an Archbishop or Prime Minister.

I have sometimes thought that he was too easily convinced of the abilities of his acquaintances, but this is an amiable fault. He was occasionally critical of people whom he thought had behaved badly or were obstructing some geophysical project, but for the most part he remembered only help and kindnesses received from others. Naturally, he and Lady Lenox-Conyngham made a host of friends, many of whom count having met them as among the most fortunate events of their lives.

In writing this notice I have made extensive use of notes he left with the Royal Society, and have had the benefit of talks with Lady Lenox-Conyngham and Miss Lenox-Conyngham and of comments on the MS. by Dr de Graaff Hunter. In his notes Lenox-Conyngham says, 'I have not published anything except Reports in the publications of the Survey of India'; a bibliography given overleaf shows this to be far from true, a more thorough search would probably reveal other publications, but I think it unlikely that I have missed anything of importance.

E. C. BULLARD

BIBLIOGRAPHY

1901.* Electro-Telegraphic Longitude operations executed during the years 1894-95-96. *Acct. ops. trig. Surv. India*, 17.

1905. Pendulum operations. *Extr. Offrs. Surv. India*, 1903-1904, 129-142.

1907. Pendulum operations. *Extr. Offrs. Surv. India*, 1904-1905, 75-85.

1908. Pendulum operations. *Extr. Offrs. Surv. India*, 1905-1906, 76-87.

1909. The pendulum operations. *Extr. Offrs. Surv. India*, 1906-1907, 87-90.

1908. The pendulum operations in India, 1903-1907. *Prof. Pap. Surv. India*, No. 10.

1912. Account of a determination of the coefficient of expansion of nickel-steel wires. *Miscellaneous publications relating to the measurement of geodetic bases by the Jäderin invar. apparatus*, pp. 1-5, Survey of India, Dehra Dun.

1912. On the experimental measurement of a base line. *Miscellaneous publications relating to the measurement of geodetic bases by the Jäderin invar. apparatus*, pp. 5-9, Survey of India, Dehra Dun.

1913. *Rec. Surv. India*, 1910-1911 (Pt. 2, Astronomical Latitudes), **2**, 24-29.

1913. *Rec. Surv. India* (Pt. 5, Levelling), **2**, 66-86.

1913. *Rec. Surv. India*, 1911-1912 (Pt. 5, Levelling), **3**, 88-109.

1913. Note in reply to Mr. Hayden's paper on the relationship of the Himalayas to the Indo-Gangetic plain and the Indian Peninsula. *Rec. Surv. India*, **5**, 161-164.

1915. Introduction to 'The pendulum operations in India and Burma 1908-1913'. *Prof. Pap. Surv. India*, No. 15, pp. 1-6.

1917. Report on the levelling carried out to watch the behaviour of bench-marks fixed to trees in the compound of the Trigonometrical Survey Office, Dehra Dun, during the period April 1914 to April 1917. *Rec. Surv. India*, **11**, 95-96.

1917. Note on Basevi's pendulum observation at Moré. *Rec. Surv. India*, **11**, 97-99.

1923. The origin and development of the theory of isostasy. *Rep. Brit. Ass.* 533-541.

1923. The proposed determination of primary longitudes by international co-operation. *Geogr. J.* **68**, 108-123.

1925. (With F. A. Potts.) The great barrier reef. *Geogr. J.* **70**, 314-334.

1929. The Cambridge pendulum apparatus. *Geogr. J.* **73**, 326-341.

1929. Note on the bathometer. *Geogr. J.* **74**, 58, and 59-60.

1932. Deflections of the plumb-line and gravity anomalies. *Conf. Emp. Surv. Off. 1931 Pap. Proc.* 63-74. (H.M.S.O.)

1936. Orthomorphic. *Emp. Surv. Rev.* **3**, 309-310.

1937. Montserrat and the West Indian volcanoes. *Proc. Roy. Instn.* **29**, 607-629.

1943. Colonel Sir Sidney Burrard, Bart., K.C.S.I., F.R.S. *Emp. Surv. Rev.* **7**, 146-155.

1943. Colonel Sir Sidney G. Burrard, Bart., K.C.S.I., F.R.S. *Roy. Engrs' J.* **57**, 289-291.

1944. Sir Sidney Burrard. *Obit. Not. Roy. Soc.* **4**, 507-521.

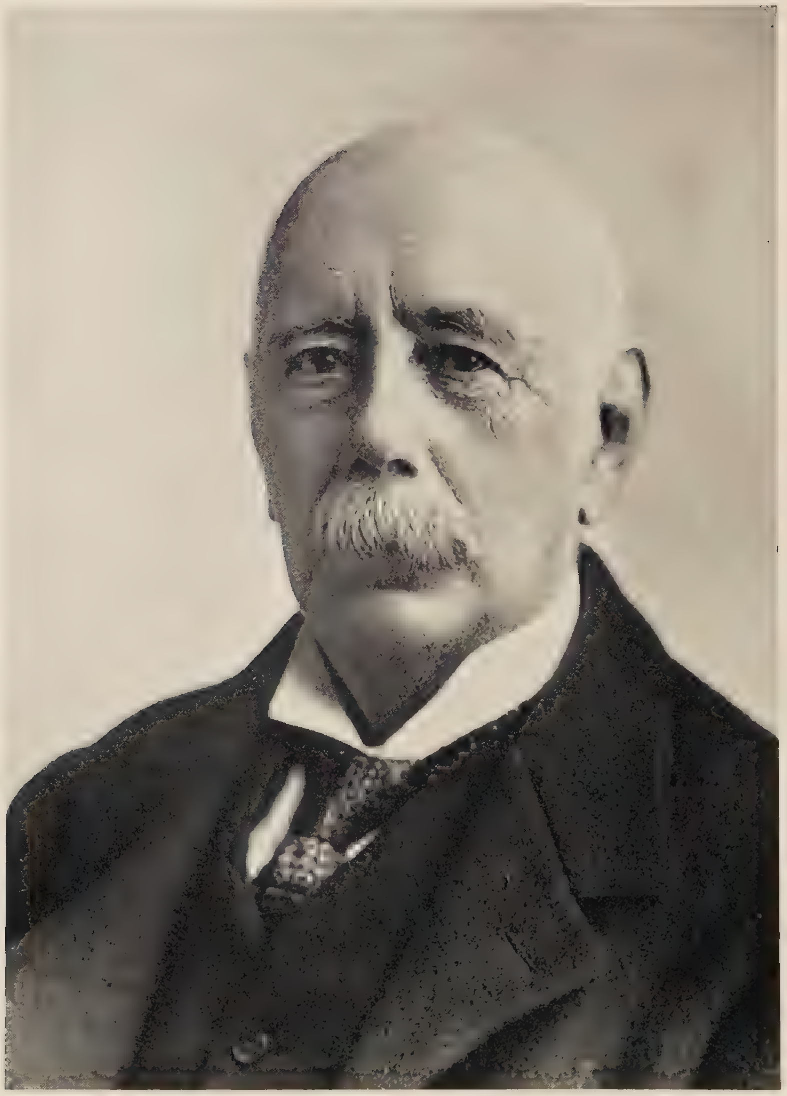
1944. Obituary notice of Sidney Gerald Burrard. *Mon. Not. R. Astr. Soc.* **104**, 86-88.

The Geographical Journal vols. 33, 57, 64, 65, 67, 69, 71, 77, 78, 87, 93 and 97 contain contributions by Lenox-Conyngham to discussions.

I have manuscripts of obituary notices on Sir Henry Lyons and C. Lallamond by Lenox-Conyngham, but have been unable to find where they were published.

---

* Lenox-Conyngham's name does not appear on the title page of this volume, but the preface states 'the descriptive chapters, the diagrams and the tabular arrangement . . . are due to Major Lenox-Conyngham.'



H N Ridley

HENRY NICHOLAS RIDLEY

1855-1956

When anyone attains to over a century at the time of their death it is inevitable that there should be few, even of near contemporaries, who can attest to the personal qualities that marked the individual in the full vigour of his career, but this deficiency is less marked with respect to H. N. Ridley, since he retained so much of his vigour and particularly his mental zest until almost the closing years of his life and, moreover, he fortunately left a personal record that has been invaluable in reconstructing the beginnings of his career. The writer is also indebted to his widow and to Mr I. H. Burkill, who succeeded Ridley as Director of the Botanic Gardens, Straits Settlements, from 1912 to 1925, for help in relation to the earlier years.

Henry Nicholas Ridley was born on 10 December 1855 at the period when plant physiology under the influence of Dutrochet, Leibig and Boussingault was beginning to affect the general approach to the study of plants, and when Hofmeister had conclusively shown the nature of the sexual process in phanerogams and had given an impetus to the study of the life history of plants. Thus Ridley grew up in a period of great intellectual activity but, above all, in its influence on the young men—the *Origin of Species* appeared when he was four years old. By the time Ridley was a student this had become a vitalizing influence in botanical thought and outlook, which gave a new significance to the study of modes of dispersal of seeds and fruits and to the geographical distribution of plants, that had a lifelong effect on Ridley's concepts and interests.

Henry Nicholas was the third child in a family of five and the second son. His birth took place at West Harling Hall in Norfolk, a county of great natural history traditions. His father was a clergyman, Oliver Matthew Ridley, son of the Rev. H. C. Ridley of Hambledon. His mother was Louisa Pole Stuart, daughter of William Stuart of Aldenham Abbey in Hertfordshire. Through his mother, Ridley was the great-great-grandson of that Marquis of Bute who, though not in name, was in effect the first scientific Director of the Royal Botanic Gardens at Kew and whatever his shortcoming as a politician, was a very competent botanist. Ridley's mother died when Henry was only three years old, but she had a great interest in mineralogy, with a fine private collection now in the possession of the British Museum, and was also interested in birds and shells, so that it was doubtless from her and her ancestors that the bent for natural history of Henry and his brother Stuart was derived. Stuart Oliver Ridley, his elder brother, was at one time an assistant at the British Museum in charge of sponges and corals and was a collector of insects. He had studied under Haeckel and at Oxford, but later he took holy orders and relinquished his natural history activities. Henry's younger brother Charles also took holy orders and of his two sisters the younger became a Deaconess in the East End of London.

On his father's side Ridley's ancestors included Bishop Nicholas Ridley (1500-1558), who was burnt in the reign of Queen Mary, Dr Mark Ridley who, towards the end of the 16th Century was a physician to the Czar, and Dr Humphrey Ridley who in the 17th Century wrote on the anatomy of the brain. His great-grandfather was the politician Sir Matthew White Ridley, who died in 1813 at the age of 68. His grandfather was Canon H. C. Ridley of Hambledon who was only 52 years of age when he died, but Ridley's father attained the age of ninety-three years, thus exhibiting some measure of the longevity that Henry so wonderfully displayed.

When Henry was five years old the family moved from Norfolk to Cobham in Kent, where his father remained as Vicar for sixteen years and here the bent for natural history began to manifest itself, the more so perhaps that Henry's health was at this period very indifferent, so that the more strenuous amusements of boys of his age had less than their usual attractions. He recalled that a vivid memory was his first visit to London when he was seven years of age and was taken to the Zoological Gardens where he saw the python on its eggs and where one of the keepers put a lion cub, nearly as big as himself, into his arms.

When nine years old, Henry went to a private school of about forty boys at Southborough, near Tunbridge Wells. Here there were lessons in Latin and Greek, mathematics, history, geography and religion, but the master knew nothing of natural science and indeed Henry recalls that, despite his youth, he was the acknowledged authority on natural history in the school.

Four years later, at the age of thirteen, Henry went to Haileybury, where his brother Stuart Oliver was already a pupil. Both the brothers collected Lepidoptera, but Henry also collected beetles, encouraged to do so by a good named collection of Coleoptera in the School Museum. He records that they paid no attention to other groups because there was no means of identifying them, an interesting commentary on the value of access to properly named local collections.

There was at that period at Haileybury no chemical laboratory and very few scientific books and, apart from mathematics, no master taught science, but natural history was encouraged by the award of prizes for collections made in the summer holidays. It was not till the end of Ridley's time at Haileybury that a school natural history society was started, but it would appear that the attitude towards his bent was sympathetic and enlightened, since he was excused cricket in the summer and permitted to spend the time collecting. Just before he left he prepared a local list of mammals and Coleoptera.

For three years, after leaving Haileybury, Ridley studied anatomy and physiology with a private tutor, the Rev. W. Hill, of Medmenham on the Thames near Henley, who had obtained a first class in science at Oxford. During this period Ridley stated that he dissected every kind of animal he could obtain. He entered Exeter College, Oxford, in 1875, where after passing in chemistry and physics he studied anatomy and physiology, entomology and geology. Amongst his teachers were Professor Rolleston, Ray Lankester and Professors Prestwich and Westwood. He also had a few lectures on botany from Professor Marmaduke Lawson. It is quite obvious that at this time his interests were zoological and geological rather than botanical and indeed one gets the impression that the subject of botany was not then taught in a way that presented any particular attractions.

Ridley obtained a second class in science in 1878 and was awarded the Burdett-Coutts Scholarship in geology in the following year. After taking his degree he spent two years arranging fossils in the museum and collected and studied the much-neglected Collembola, the Thysanura, Myriapoda and other groups, describing a new species of marine copepod, *Dardicola antheae* Ridley. He was on the look-out for a paid post and, as these were comparatively few, he had deliberately covered a wide range in the hope that if a vacancy offered he might have the requisite qualification to fill it, but one should note that he placed it on record that his weakest subject was then botany. Nevertheless it was in the Botanical Department of the British Museum that Ridley obtained his first appointment in 1880 and remained there for a period of eight years, during which he published a number of papers both zoological and botanical, the latter comprising contributions both to British and tropical botany. Thus in 1884 he was describing new and rare plants from Madagascar and Angola, whilst in the year following we find him recording the occurrence of *Schoenus ferrugineus* and *Carex salina* in Britain. The breadth of his interests at this period is manifest alike in his contributions to botanical and zoological literature and in 1886 we find him collaborating with his brother in a paper published in *The Zoologist* on animal life in high latitudes.

Although from this time onwards his activities became more and more directed towards botanical studies his interest in animal life persisted and was retained to the end, for in his later years one of his greatest enjoyments was to watch the birds in Kew Gardens.

During the period when he was at the British Museum Ridley was patently developing what became his abiding interest in the distribution of plants, for with Dr Fawcett he actively studied the flora of Dorset with a view to helping Mansel-Pleydell's work on that subject, and Ridley also studied the flora of S.E. Wales to assist H. C. Watson to fill gaps in the knowledge of the plant life of this area for the second edition of his *Topographical botany* that was published in 1883. At this time Ridley's travels seldom extended beyond the British Isles though he visited both Norway and Switzerland. However in 1887 he was sent by the Royal Society on an expedition to Brazil with an Edinburgh zoologist, G. A. Ramage, where in the Pernambuco area they collected animals, plants and minerals, and also in the Island of Fernando Naronha, their main objective, where mapping as well as collecting was undertaken. They were joined at Para by the Rev. T. S. Lea, an old schoolfellow of Ridley's.

In 1888 Ridley was offered and accepted the post of Director of the Gardens and forests of the Straits Settlements at Singapore. These Gardens were established by the Agri-Horticultural Society in 1859, but following a succession of financial crises they were taken over by the Government. Originally intended as an amenity Park rather than a scientific Institution, in which the band of the Regiment stationed at Singapore would provide music, it contained a zoological garden, which activity, prior to Ridley's advent, had become limited to a collection of birds as the maintenance of the other animals was a contributing cause of the financial deficit. At the time of Ridley's appointment the botanical collection was meagre, only some 500 species being then in cultivation and this included a small proportion of economic plants. The entire area was about eighty acres for which the financial allocation was inadequate to provide sufficient labour for proper maintenance, still less for development. To assist in the provision of funds plants were sold from the gardens to members of the Society and this applied to the economic species. Thus, before Ridley's arrival, the facilities and contacts for the distribution of economic plants were already established and his development of these was to play an important role in the subsequent distribution of the rubber plants derived from those sent out from Kew. Ownership of the Gardens passed to the Government in 1874 and not only was the botanical aspect of its activities developed but the economic garden was established. Ridley was entrusted with a threefold charge. The garden itself, the economic garden and a preliminary forest survey. It was this last activity which attracted him most, involving as it inevitably must, travel and exploration of unknown country with all its appeal to a man who was primarily a naturalist, and he was always ready to visit planters to give them his advice, provided it afforded him an opportunity of going into the jungle.

Thus Ridley carried out many explorations throughout his Directorship in the Malay Peninsula itself, often to parts previously unknown, collecting living plants for the gardens and dried plants for the Herbarium which he established at the Botanical Gardens, and amassing the information and material upon which his *Flora of the Malay Peninsula* was based. But in addition his travels and collections extended to adjacent areas. Thus to the northward he explored Southernmost Siam in 1911 and in Burma in 1912. Southwards of Malaya, he visited Borneo and Sumatra in 1897, Java in 1915, and Sarawak four times, in 1903, 1904, 1914 and 1915. Further afield his travels led him to Christmas and Keeling Islands in 1890-91, to Egypt and India in 1912, and to Jamaica in 1916. Wherever he went he collected plants and the living material at Kew was continually enriched by him, but as those who talked to Ridley in his later years can testify, his observations ranged over a wide field and his retentive memory illumined his reminiscences with faunistic and anthropological details that were symptomatic of his catholic interests.

At Singapore the relations of the Gardens with the local planters made the task of new introductions less difficult than it might otherwise have been, but there is the understandable caution in respect to the economics of an unfamiliar subject of culture and the acquisition of the special technique for a new crop that alike raise obstacles against its ready adoption, even where the prospects of ultimate success are hopeful.

The origin of plantation rubber is an oft told tale though its history has not infrequently been over dramatized and the part played by individuals misrepresented. The credit for the conception of bringing the rubber tree from the New World to the Old belongs to Sir Clement Markham and to Sir Joseph Hooker, the then Director of the Royal Botanic Gardens. H. A. Wickham had studied *Hevea brasiliensis*, the Para rubber tree, in its native habitat in Brazil and in 1875 at the instance of Hooker brought some seventy thousand seeds to Kew. These seeds are oily in character and, like most such, lose their viability rather rapidly. Wickham therefore did all he could to hasten their despatch, including the feasting of the local Customs officials in the hope of accelerating the normal leisurely procedure and this has been quite unjustifiably dramatized into a smuggling achievement, whereas there was in fact no secret as to what he was bringing out with him. Despite all Wickham's efforts to hasten matters and the propagating skill of the Kew staff, out of this vast number of seeds only about 4 per cent germinated, nevertheless approximately nineteen hundred young plants were despatched from Kew, in the miniature greenhouses that had been developed there for such transport, known as 'Wardian Cases'. Most of these seedlings were landed at Ceylon and some were taken to Singapore before Ridley became Director. It was, however, Ridley's faith in the ultimate value of the Para rubber tree in Malaya that led him to devote his energies to the task of persuading planters to experiment with the new crop and to persevere in overcoming the difficulties of its culture. Indeed to Ridley more than anyone else is due credit for the establishment of the rubber plantation industry and this was acknowledged in 1914 by the award to him of the Rubber Planters Association Gold Medal. Moreover fourteen years later his services to plant introductions generally and to that of rubber especially were signally recognized from America by the award of the Frank Meyer Medal.

Ridley was a keen observer and a great naturalist whose outlook was a wide one stimulated by a diversity of interests and informed by a broad scientific experience. Although his professional career was, throughout the greater part of his life, a botanical one, we have seen that in his early years his occupations were primarily zoological and geological. It was perhaps this fact and the breadth of the interests he maintained throughout his long life that led to the attraction for him of the problems of dispersal. It is manifest that the integration of botanical and ornithological knowledge, which the study of seed and fruit dissemination by birds requires, made to his mind a special appeal. Thus we find Ridley carrying out careful experiments on feeding seeds and fruits to the birds of Singapore in order to determine the interval of time that elapsed between feeding and the evacuation of the seeds, since he rightly appreciated that upon this would in considerable measure depend the distances to which these different kinds of birds would be effective agents for dispersal. So too he paid attention to the seed contents of the droppings of mammals and their habits of evacuation. In this connexion he observed that the Malayan rhinoceros *R. sumatrensis*, frequently follows a regular route that may occupy a month or more of travel and upon this route defecation takes place at special spots. Ridley noted how, when a hut had been placed over one of these the rhinoceros went through the hut like a living tank but left his visiting card as usual. So, although seeds may thus be deposited great distances from the parent plant, where the animal was feeding, yet the dispersal was not promiscuous. Again we find this theme emphasized in his observations on how the seeds of the climber *Willughbeia* tend to be shed by monkeys around the base of trees that are suitable for their support because, when seizing a fine fruit, the monkey climbs into the fork of a big tree to escape from the predatory instincts of his companions and devouring it hurriedly strews the seeds onto the ground around the tree base.

Ridley's interest in the animal-plant relations is further shown in his paper on myrmecophilous plants in which he found every gradation in the relation between the ants and the host-plant from what he regarded as an accidental association, exemplified by *Ficus irregularis* to an actually mutually adaptive symbiosis as with *Macaranga triloba*.

It is not without significance that one of Ridley's two most important works dealt with the dispersal of plants and that of its 744 pages nearly half were devoted to dispersal by animals. Wind dispersal also particularly attracted his attention, probably owing to his having so often witnessed it in action, and also because this mode of transport, like dispersal by birds, often played a large part in the colonization of islands, a problem which perpetually intrigued him.

His other most significant publication was the five volume work on *The Flora of the Malay Peninsula*. This was a pioneering contribution of great importance, based upon his extended explorations of that area, in the course of which he built up the important Herbarium at the Botanical Gardens. The number of new species which he discovered was large and it has been aptly stated that he knew his plants as a familiar friend, rather than as a critical botanist. Critical determination is of course essential to the advancement of science, but the mentality that is obsessed with the discrimination of differences between organisms is not infrequently a mere collector of information, however valuable this may be to enable others to garner the fruits of knowledge. To such taxonomists the discovery of the morphological criteria to distinguish between two allied organisms may be an end in itself and is to be compared with enthusiasm for the discovery of philatelic errors. Ridley was not in this category but was primarily interested in the biology of organisms and only secondarily with the morphological distinctions between them, the more so that he was so conversant with many species that he recognized them by integrated assessment, without resort to analytical diagnosis. It is in the light of such a conception that we must appreciate the merits, and any shortcomings, of the Malayan *Flora*, which he produced. Subsequent work has shown the need for appreciable revision but no one could fairly doubt the importance of the pioneer efforts which Ridley made in the study of the plants of that region, despite his uncritical assessments.

Dr Holttum in his work on the Orchidaceae of Malaya which embraces nearly one thousand species, refers to the fact that a large number of these species were first described by Ridley, although 'unfortunately his descriptions of them are often unsatisfactory'. Ridley's account of the ferns of the Malay Peninsula treats of some 420 species which represent about 82 per cent of the species now known from this area. The region comprises about 52000 square miles, much of it difficult country and it is no small tribute to Ridley's energy and observational acumen that so high a proportion of the vascular plants of Malaya that have so far been recorded were first found by him. The flora of the region is one of great botanical interest with a high proportion of endemics especially in some genera, as for example *Eugenia* represented by 122 species of which no less than 100 are endemic and *Didymocarpus* with 53 species of which 46 are endemic to this area.

Ridley's knowledge of the Malay flora led him to question the general applicability of Willis's age and area hypothesis and he pointed out also that many weeds show no such correlation.

The interest which Ridley took in the applied aspects of the study of plants was shown not only by his fostering of the early stages of the rubber industry but also by the numerous notes and papers which he wrote on the economic aspects of botany, including the book *Spices* which was published in 1912. He called attention to what he believed to be a most interesting feature, namely that a great number of the Malay fruit trees are not known in the wild state. He believed this to be true of the durian, the mangosteen, the rambutan, the pulasan, betel nut and many others, whilst his observation of the habits of the natives enabled him to provide a plausible explanation for the extinction of these trees from the truly wild stations and their preservation in cultivated areas.

Ridley initiated the publication of the *Agricultural Bulletin* first as an annual issue and subsequently as a monthly, and in this he personally provided numerous articles on economic botany, treating of such subjects as Malayan timbers, fibres, fruits and drugs, and he studied the pests and diseases of the plants producing these. Ridley himself carried out experiments on the cultivation and tapping of rubber and in the face of opposition persistently emphasized his belief that the plantation culture of Para rubber was both practicable and economically desirable despite the fact that the early trials of rubber in Ceylon had been disappointing.

It was Ridley also who in 1907 advocated the possibilities of the oil palm, *Elaeis guinensis*, a native of tropical Africa, as a plantation crop. The Dutch had already been experimenting with this and subsequent events fully justified their own and Ridley's confidence in its future.

One group of plants that was particularly dear to Ridley's heart was that of the orchids and of these he collected and described more than two hundred which were new to science. Other families to the knowledge of which he added extensively were the Gesneraceae, the Araceae and the Zingiberaceae. Amongst the many species discovered by Ridley a number were unique, as for example the Alismataceous plant which he found growing in the mud of a forest in Selangor, since cleared, which the late Dr Stapf described as representing a new genus *Ranalisma*, but of which the specimens collected by Ridley remain the sole examples. Ridley's partiality to the Monocotyledons had its origin in his early days at South Kensington, where he succeeded Trimen and was given charge of this group with which he quickly made himself conversant.

Ridley was elected to the Fellowship of the Royal Society in 1907, four years before he retired from the Directorship of the Singapore Botanic Gardens. He then came to live in the Cumberland Road at Kew. His proximity to the Royal Botanic Gardens enabled him conveniently to spend a considerable part of his time in the Herbarium during the next fourteen years whilst engaged upon his *Flora of the Malay Peninsula*, of which the fifth and last volume appeared in 1925. After the completion of this *magnum opus* he continued his taxonomic studies from this region up to the time of the outbreak of the Second World War and to write his other important book on *Plant dispersal*.

Throughout this period he was a constant attender of the meetings of the Linnean Society, frequently participating in the discussions to which his wide experiences added interest and information. He served twice on the Council of this Society, of which he was a Vice-President in 1914. He was also a member of the Linnean Society Dining Club, where his powers as a conversationalist and his vivid reminiscences often enlivened the proceedings. He was awarded the Linnean Society's Gold Medal in 1950.

Ridley had attained the age of eighty-four when the second World War came upon us and with the war-time restrictions and decreasing physical vigour his activities, although still far from negligible, considerably diminished. Until he was ninety-six he continued to visit the Kew Herbarium almost daily and up to 1945 kept the records of the bird life in the Gardens and assiduously made notes and drawings of the magnolias that fruited there.

In his eighty-second year Ridley married Lilian Doran. Earlier he had suffered from sprue but in later years his only ailment was a liability to bronchial attacks during the winter months and these became increasingly severe. It was largely due to the unremitting care and nursing of his wife and her sister that Ridley survived, as was his great hope, to celebrate the centenary of his birth.

The appended bibliography shows how numerous were his published contributions and also the extensive range of subjects in which he took a serious interest. As further emphasizing this latter aspect we may note that he was the last surviving founder-member of the Society for Psychical Research (1882) and served on two of its committees, namely that on 'Mesmerism' and that on the 'Reichenbach phenomena' of which reports were published in the Proceedings of the Society.

Apart from books his botanical publications both pure and applied numbered nearly 350, in addition there were 68 on zoological subjects, 8 on geological and mineralogical topics, 12 of a medical character, 5 treating of ethnological matters and fourteen biographical notes. Many of these it is true are but short notes, often however embodying important observations, but the total output, from the time he was a schoolboy of seventeen to when he was an old man of ninety-one, is an impressive monument to his industry and the alertness of his mind. It was a century of zestful living and gainful occupation for one who, despite his long life, found this all too short for what he was always impatient to accomplish.

Ridley's hundredth anniversary was the occasion of various presentations and numerous tributes from home and overseas and a special message was conveyed to him from the President and Council of the Royal Society. He died on 24 October 1956, less than two months short of his 101st birthday.

For the excellent photograph I am indebted to Mr Gerald Atkinson. This shows well his most striking feature, namely the eyes, the vivacious expression of which impressed the beholder at once as indicative of a keen observer. The writer would also wish to acknowledge his debt to Mr H. S. Marshall for assistance in checking the extensive bibliography.

E. J. SALISBURY

BIBLIOGRAPHY

BOTANICAL

1880. Spring-flowering form of *Colchicum autumnale*. *J. Bot. Lond.* **18**, 185.
1881. A new variety of *Carex pilulifera* L. *J. Bot. Lond.* **19**, 97.
1881. Notes on Radnorshire plants. *J. Bot. Lond.* **19**, 170.
1882. (With W. FAWCETT.) Additions to the flora of Dorset. *J. Bot. Lond.* **20**, 246.
1883. On the generic names *Dantia* and *Prouvenzalia*. *J. Bot. Lond.* **21**, 349.
1884. Cyperaceae novae. *J. Bot. Lond.* **22**, 15.
1884. Kerry plants. *J. Bot. Lond.* **22**, 91.
1884. Shetland plants. *J. Bot. Lond.* **22**, 301.
1884. Bees and *Erica cinerea*. *J. Bot. Lond.* **22**, 302.
1884. Additions to 'Topographical botany'. *J. Bot. Lond.* **22**, 377.
1884. Cyperaceae of the West Coast of Africa in the Welwitsch Herbarium. *Trans. Linn. Soc. Lond.* (*Bot.*) (Ser. 2), **2**, 121.

1884. A new series of *Albuca* from Aden. *J. Bot. Lond.* **22**, 370.
1884. A new Bornean orchid. *J. Bot. Lond.* **22**, 333.
1884. On *Didymoplexis sylvatica* (*Leucorchis sylvatica Blume*). *J. Bot. Lond.* **22**, 345.
1884. Teratological notes on plants.—I. *J. Linn. Soc.* (*Bot.*), **20**, 45.
1884. Descriptions and notes on new or rare monocotyledonous plants from Madagascar, with one from Angola. *J. Linn. Soc.* (*Bot.*), **20**, 329.
1885. A new *Carex* from Sumatra. *J. Bot. Lond.* **23**, 35.
1885. A new *Dendrobium* from Siam. *J. Bot. Lond.* **23**, 123.
1885. A new *Habenaria* from Brazil. *J. Bot. Lond.* **23**, 170.
1885. *Crocus korolokowi* in Afghanistan. *J. Bot. Lond.* **23**, 185.
1885. Peloria in *Habenaria bifolia* Br. *J. Bot. Lond.* **23**, 218.
1885. Two new British plants. *J. Bot. Lond.* **23**, 289.
1885. On a new species of *Gussonea*. *J. Bot. Lond.* **23**, 310.
1885. Notes on British Rubi. *J. Bot. Lond.* **23**, 370.
1885. *Angraecum glomeratum*, n. sp. *Gdnrs' Chron.* **24**, 678.
1885. Monocotyledons of Timor (*in* FORBES, H. O. *Wanderings of a naturalist*, p. 518.)
1886. *Habenaria albida* in Brecon. *J. Bot. Lond.* **24**, 285.
1886. Notes on the orchids of Tropical Africa. *J. Bot. Lond.* **24**, 291.
1886. A new Amorphophallus from Gambia. *J. Bot. Lond.* **24**, 305.
1886. On the monocotyledonous plants of New Guinea collected by Mr H. O. Forbes. *J. Bot. Lond.* **24**, 321.
1886. On Dr Fox's collection of orchids from Madagascar, along with some obtained by the Rev. R. Baron, F.L.S., from the same island. *J. Linn. Soc.* (*Bot.*), **22**, 116.
1886. On the freshwater Hydrocharideae of Africa and its Islands. *J. Linn. Soc.* (*Bot.*), **22**, 232
1886. A monograph of the genus *Liparis*. *J. Linn. Soc.* (*Bot.*), **22**, 244.
1887. The botany of the Roraima Expedition. Orchidaceae and Cyperaceae. *Trans. Linn. Soc.* (*Bot.*), **2**, 281, 287.
1887. [Notes on new species in the Families of Orchideae, Scitamineae, Cyperaceae]. *Bol. Soc. Broteria.* **5**, 198, 202, 208.
1887. *Burmannia bicolor* Mart. in Africa. *J. Bot. Lond.* **25**, 85.
1887. Angolan *Scitamineae*. *J. Bot. Lond.* **25**, 129.
1888. On a new genus of Orchideae from the Island of St Thomas, West Africa. *J. Linn. Soc.* (*Bot.*), **24**, 197.
1888. A revision of the genera *Microstylis* and *Malaxis*. *J. Linn. Soc.* (*Bot.*), **24**, 308.
1888. Notes on self-fertilization and cleistogamy in orchids. *J. Linn. Soc.* (*Bot.*), **24**, 389.
1888. On the foliar organs of a new species of *Utricularia* from St Thomas, West Africa. *Ann. Bot. Lond.* **2**, 305.
1888. *Ponthieva grandiflora* Ridl. n. sp. *Gdnrs' Chron.* (3) **3**, 264.
1888. Proliferous orchids. *Gdnrs' Chron.* (3) **4**, 676.
1890. *Calanthe* (*S. Prepanthe*) *rubens* Ridley n. sp. *Gdnrs' Chron.* (3) **7**, 576.
1890. On the method of fertilization of *Bulbophyllum macranthum*. *Ann. Bot. Lond.* **4**, 327.
1890. Notes on the botany of Fernando Noronha. *J. Linn. Soc.* (*Bot.*), **27**, 1.
1890. The Burmanniaceae of the Malay Peninsula. *J. Straits Br. Asiat. Soc.* **22**, 331.
1890. On the so-called Tiger's Milk 'Susu Rimau' of the Malays. *J. Straits Br. Asiat. Soc.* **22**, 341.
1890. *Matonia pectinata* in the Karimen isles. *J. Straits Br. Asiat. Soc.* **22**, 430.
1891. The grasses and sedges of the Malay Peninsula. *J. Straits Br. Asiat. Soc.* **23**, 430.
1891. A day at Christmas Island. *J. Straits Br. Asiat. Soc.* **23**, 123.
1891. On the occurrence of a rhododendron at Singapore. *J. Straits Br. Asiat. Soc.* **23**, 144.
1891. *Pogonia punctata*, Bl., at Singapore. *J. Straits Br. Asiat. Soc.* **23**, 146.
1891. The genus *Bromheadia*. *J. Linn. Soc.* (*Bot.*), **28**, 331.
1891. On two new genera of orchids from the East Indies. *J. Linn. Soc.* (*Bot.*), **28**, 340.
1891. On the occurrence of *Petrosavia* in Perak. *J. Straits Br. Asiat. Soc.* **24**, 170.

1893. Vanda Miss Joaquim [inter V. Hookerianam et V. Teretem proles hybrida]. *Gdnrs' Chron.* (3) **13**, 740.
1893. On the flora of the Eastern Coast of the Malay Peninsula. *Trans. Linn. Soc. Lond.* (*Bot.*) **3**, 267.
1894. On the dispersal of seeds by mammals. *J. Straits Br. Asiat. Soc.* **25**, 11.
1894. Account of a trip up the Pahang, Tembeling, and Tahan Rivers, and an attempt to reach Gunong Tahan. *J. Straits Br. Asiat. Soc.* **25**, 33.
1894. Journey on the Sembrong River. List of plants collected. *J. Straits Br. Asiat. Soc.* **26**, 25.
1894. Journal of a voyage from India to Siam and Malacca in 1779 by Dr J. G. Koenig. *J. Straits. Br. Asiat. Soc.* **26**, 58.
[*Note:*—There is no indication in the text that this translation is by Ridley.]
1894. *Pothos flexuosus*' Hort. *Gdnrs' Chron.* (3) **15**, 526.
1894. Flora of Mount Kinabalu. Orchideae. *Trans. Linn. Soc. Lond.* (*Bot.*) **4**, 233.
1894. The camphor tree and camphor language of Johore. *J. Straits Br. Asiat. Soc.* **26**, 35.
[*Note:*—Article attributed to H. Lake and H. J. Kelsall, but signed H.N.R.]
1895. *Linospadix micholitzii* Ridley n. sp. *Gdnrs' Chron.* (3), **18**, 262.
1895. *Peperomia malaccensis* Ridl. n. sp. *Kew Bull.* p. 185.
1895. Two new species of *Thismia*. *Ann. Bot. Lond.* **9**, 323.
1895. Utriculariae of the Malay Peninsula. *J. Bot. Lond.* **33**, 10.
1895. On two new species of Clerodendron. *J. Bot. Lond.* **33**, 42.
1896. The *Orchideae* and *Apostasiaceae* of the Malay Peninsula. *J. Linn. Soc.* (*Bot.*), **32**, 213.
1896. Cyrtandraceae Malayenses. *J. Linn. Soc.* (*Bot.*), **32**, 497.
1896. The Dracaenas of the Malay Peninsula. *J. Bot. Lond.* **34**, 162.
1896. A new genus of Commelinaceae. *J. Bot. Lond.* **34**, 329.
1896. Dispersal of seeds by birds. *Natural Science*, **8**, 186.
1896. The forests of Selangor. *Selangor Journal*, **4**, 444.
1897. Malay plant names. *J. Straits Br. Asiat. Soc.* **30**, 31.
1897. *Calanthe vestita* Lindl. in Selangor. *J. Straits Br. Asiat. Soc.* **30**, 311.
1897. The Jering fruit (*Pithecolobium lobatum* Benth.). *Proc. Asiat. Soc. Beng.* p. 147.
1898. The *Peliosanthes* of the Malay Peninsula. *J. Straits Br. Asiat. Soc.* **31**, 73.
1898. New Malayan orchids. *J. Bot. Lond.* **36**, 210.
1898. *Livistona woodfordii*, Ridl. n. sp. *Gdnrs' Chron.* (3), **23**, 177.
1898. *Ptychosperma sanderiana*, Ridl. n. sp. *Gdnrs' Chron.* (3), **23**, 330.
1898. New species of *Entada* from Singapore and Borneo. *J. Asiat. Soc. Beng.* **67**, 305.
1899. The Scitamineae of the Malay Peninsula. *J. Straits Br. Asiat. Soc.* **32**, 85.
1899. [Conifers in the Malay Peninsula.] [*In* Colonial Notes.] *Gdnrs' Chron.* (3), **25**, 351.
1900. Plants collected at Penrissen. *J. Straits Br. Asiat. Soc.* **33**, 22.
1900. The flora of Singapore. *J. Straits Br. Asiat. Soc.* **33**, 27.
1900. A botanical excursion to Gunong Jerai (Kedah Peak). *J. Straits Br. Asiat. Soc.* **34**, 23.
1900. Dammar and wood oil. *J. Straits Br. Asiat. Soc.* **34**, 89.
1900. Some new Eastern gingers. *J. Straits Br. Asiat. Soc.* **34**, 97.
[*Note:*—Not attributed to Ridley, but reference is made to a previous paper.]
1900. Botanical notes on Malayan gingers. *J. Straits Br. Asiat. Soc.* **34**, 99.
[See note to entry immediately above.]
1900. New Malayan plants. *J. Bot. Lond.* **38**, 68.
1900. Native rubbers of the Malay Peninsula. *Agric. Bull. Malay Pen.* p. 239.
1900. Injurious fungi. *Agric. Bull. Malay Pen.* p. 284.
1900. *Habenaria columbae* Ridl. n. sp. *Gdnrs' Chron.* (3), **28**, 390.
1901. The flora of Mount Ophir. *J. Straits Br. Asiat. Soc.* **36**, 1.
1901. Garu and Chandan. *J. Straits Br. Asiat. Soc.* **36**, 73.
1901. Supplementary notes on the flora of Singapore. *J. Straits Br. Asiat. Soc.* **36**, 84.
1901-2. The timbers of the Malay Peninsula. *Agric. Bull. Str. Malay St.* **1**, 1.
1902. The dissemination of seeds by natural methods. *Agric. Bull. Str. Malay St.* **1**, 153.
1902. *Ficus bracteata. Agric. Bull. Str. Malay St.* **1**, 386.

1902. Sudu-sudu [*Euphorbia neriifolia*]. *Agric. Bull. Str. Malay St.* **1**, 389.
1903. Notes on a trip to Gunong Benom in Pahang—plants collected. *J. Straits Br. Asiat. Soc.* **39**, 10.
1903. New Malay orchids. *J. Straits Br. Asiat. Soc.* **39**, 71.
1903. A new *Balanophora* from Tenimber Islands. *J. Straits Br. Asiat. Soc.* **39**, 207.
1903. *Brucea Sumatrana. Agric. Bull. Str. Malay St.* **2**, 41.
1903. Rattans. *Agric. Bull. Str. Malay St.* **2**, 130, 158.
1903. The camphor tree: *Dyrobalanops aromatica. Agric. Bull. Str. Malay St.* **2**, 163.
1903. Cultivation of Renantheras. *Agric. Bull. Str. Malay St.* **2**, 243.
1903-4. The mosquito plant. *Agric. Bull. Str. Malay St.* **2**, 196, **3**, 24.
1903. The fruiting of the travellers' tree, *Ravenala Madagascariensis. Agric. Bull. Str. Malay St.* **2**, 291.
1903. *Dendrobium taurinum* var. *album. Agric. Bull. Str. Malay St.* **2**, 368.
1904. *Zalacca conferta. Agric. Bull. Str. Malay St.* **3**, 168.
1904. The palm collection of the Botanic Gardens, Singapore. *Agric. Bull. Str. Malay St.* **3**, 249.
1904. Three new Bornean palms. *Gdnrs' Chron.* (3) **35**, 50.,
1904. *Sterculia scaphigera. Agric. Bull. Str. Malay St.* **3**, 307.
1904. Weed-pests. *Agric. Bull. Str. Malay St.* **3**, 490.
1904. New Malayan plants. *J. Straits Br. Asiat. Soc.* **41**, 31.
1904. On the flowering of *Barringtonia racemosa. J. Straits Br. Asiat. Soc.* **41**, 125.
1904. Fertilization of *Webera stellulata. J. Straits B. Asiat. Soc.* **41**, 126.
1904. Insect attractions in flowers. *New Phytol.* **3**, 164.
1905. The Gesneraceae of the Malay Peninsula. *J. Straits Asiat. Soc.* **44**, 1.
1905. The aroids of Borneo. *J. Straits Br. Asiat. Soc.* **44**, 169.
1905-22. New and little-known Malayan plants. *J. Straits Br. Asiat. Soc.* **44**, 189; **49**, 11; **50**, 111; **54**, 1; **61**, 1; **68**, 11; **73**, 139; **75**, 5; **79**, 63; **82**, 167; **86**, 292.
1905. Kumus (*Shorea ciliata*). *Agric. Bull. Str. Malay St.* **4**, 63.
1905. *Grammatophyllum speciosum. Agric. Bull. Str. Malay St.* **4**, 141.
1905. The pupoi (*Connaropsis Griffithii*). *Agric. Bull. Str. Malay St.* **4**, 155.
1905. *Isoptera borneensis. Agric. Bull. Str. Malay St.* **4**, 219.
1905. Notes on the collection of aroids cultivated in the Botanic Gardens, Singapore. *Agric. Bull. Str. Malay St.* **4**, 255.
1905. On the dispersal of seeds by wind. *Ann. Bot. Lond.* **19**, 351.
1906. Note on the foliar organs of Monophyllaea. *Ann. Bot. Lond.* **20**, 213.
1906. *Litsea persella* Ridl. n. sp. *Agric. Bull. Str. Malay St.* **5**, 6.
1906. East Indian 'dragon's blood'. *Agric. Bull. Str. Malay St.* **5**, 28, 264.
1906. Betis or Malay Bilian. *Agric. Bull. Str. Malay St.* **5**, 39.
1906. Timbers and guttas of the Malay Peninsula. *Agric. Bull. Str. Malay St.* **5**, 61.
1906. *Cassia fistula. Agric. Bull. Str. Malay St.* **5**, 68.
1906. A parasitic fungus on roses [*Sphoerostilbe cinnabarina*]. *Agric. Bull. Str. Malay St.* **5**, 68.
1906. *Paspalum dilatum*, a large water grass. *Agric. Bull. Str. Malay St.* **5**, 113.
1906. Notes on the action of copper sulphate on aquatic plants. *Agric. Bull. Str. Malay St.* **5**, 403.
1906. An expedition to Christmas Island. *J. Straits Br. Asiat. Soc.* **45**, 137.
1906. Grasses and sedges of Borneo. *J. Straits Br. Asiat. Soc.* **46**, 215.
1906. Scitamineae of Borneo. *J. Straits Br. Asiat. Soc.* **46**, 229.
1906. Begonias of Borneo. *J. Straits Br. Asiat. Soc.* **46**, 247.
1906. Fertilization of *Barringtonia. J. Straits Br. Asiat. Soc.* **46**, 263.
1906. Christmas Island flora—additional notes. *J. Straits Br. Asiat. Soc.* **48**, 107.
1907. Late flowering of *Grammatophyllum. Agric. Bull. Str. Malay St.* **6**, 49.
1907. Fruiting of frangipani (*Plumiera*). *Agric. Bull. Str. Malay St.* **6**, 128.
1907. Fruiting of *Crinum northianum. Agric. Bull. Str. Malay St.* **6**, 139.
1907. Casuarinas. *Agric. Bull. Str. Malay St.* **6**, 169.

1907. Conifers cultivated in the Botanic Gardens. *Agric. Bull. Str. Malay St.* **6**, 169.
1907. Pandans. *Agric. Bull. Str. Malay St.* **6**, 241.
1907. Some notes on the acclimatization of plants. *Agric. Bull. Str. Malay St.* **6**, 415.
1907-9. Branching in palms. *Ann. Bot. Lond.* **21**, 415; **23**, 338.
1907. The Herbarium. *Agric. Bull. Str. Malay St.* **6**, 329.
1908. On a collection of plants made by H. C. Robinson and L. Wray from Gunong Tahan, Penang. *J. Linn. Soc.* (*Bot.*) **38**, 301.
1908. Notes on the acclimatization of plants, continued. *Agric. Bull. Str. Malay St.* **7**, 1.
1908. Heliconias. *Agric. Bull. Str. Malay St.* **7**, 129.
1908. Ipecacuanha. *Agric. Bull. Str. Malay St.* **7**, 134.
1908. Curious root development of *Albizzia*. *Agric. Bull. Str. Malay St.* **7**, 137.
1908. Fungus inside a bamboo. *Agric. Bull. Str. Malay St.* **7**, 230.
1908. *Acalypha fruticosa*, Forsk. *Agric. Bull. Str. Malay St.* **7**, 268.
1908. Notes on some Piscicides. *Agric. Bull. Str. Malay St.* **7**, 443.
1908. A list of ferns of the Malay Peninsula. *J. Straits Br. Asiat. Soc.* **50**, 1.
1908. The Labiates of the Malay Peninsula. *J. Straits Br. Asiat. Soc.* **50**, 105.
1909. Plants of Prince of Wales Island. *J. Straits Br. Asiat. Soc.* **53**, 49.
1909. Fruits of *Burbidgea*. *J. Straits Br. Asiat. Soc.* **53**, 175.
1909. *Mussaenda sanderiana* Ridl. n. sp. *Gdnrs' Chron.* (3), **46**, 34.
1909. The flora and botany of the Telôm and Padang Valleys. *J. F.M.S. Mus.* **4**, 1.
1909. (With T. W. Main) Ferns and their cultivation. *Agric. Bull. Str. Malay St.* **8**, 282.
1909. *Shorea leprosula*, Miq., 'Seraya' or 'Seraya Batu'. *Agric. Bull. Str. Malay St.* **8**, 364.
1909. The Palmetum, Botanic Gardens, Singapore. *Agric. Bull. Str. Malay St.* **8**, 455.
1909. Brazil-nuts. *Agric. Bull. Str. Malay St.* **8**, 551.
1909. The Scitaminae of the Philippine Islands. *Philipp. J. Sci.* **4**, 155.
1910. The black Brazil cherry, *Eugenia braziliensis*. *Agric. Bull. Str. Malay St.* **9**, 85.
1910. The abolition of the Botanic Gardens of Penang. *Agric. Bull. Str. Malay St.* **9**, 97.
1910. *Peliosanthes violacea* var. *Clarkei*, *Dendrobium spectabile*, and *Hosea lobbiana*, Ridl. *Agric. Bull. Str. Malay St.* **9**, 105.
1910. *Shorea collina* and *S. materialis* n. spp. *Agric. Bull. Str. Malay St.* **9**, 182, 183.
1910. Further notes on Malay camphor. *Agric. Bull. Str. Malay St.* **9**, 299.
1910. Symbiosis of ants and plants. *Ann. Bot. Lond.* **24**, 457.
1911. Gum Benjamin. *Agric. Bull. Str. Malay St.* **10**, 12.
1911. Chonemorpha rubber-vines. *Agric. Bull. Str. Malay St.* **10**, 146.
1911. Fasciation in *Eucharis*. *Agric. Bull. Str. Malay St.* **10**, 154.
1911. *Cinnamomum deschampsi*. *Agric. Bull. Str. Malay St.* **10**, 235.
1911. Root-shoots in *Casuarina glauca*. *Agric. Bull. Str. Malay St.* **10**, 359.
1911. A scientific expedition to Temengoh, Upper Perak. *J. Straits Br. Asiat. Soc.* **57**, 5.
1911. The flora of Lower Siam. *J. Straits Br. Asiat. Soc.* **59**, 15.
1911. An account of a botanical expedition to Lower Siam. *J. Straits Br. Asiat. Soc.* **59**, 27.
1911. The gymnosperms of the Malay Peninsula. *J. Straits Br. Asiat. Soc.* **60**, 53.
1912. New plants from Sarawak. *Sarawak Mus. J.* **1**, ii, 30.
1912. A botanical excursion to Pulau Adang. *J. Straits Br. Asiat. Soc.* **61**, 46.
1912. Some plants collected on Mr Moulton's expedition to Batu Lawi. *J. Straits Br. Asiat. Soc.* **63**, 59.
1913. Contributions to a flora of Borneo. *Sarawak Mus. J.* **1**, iii, 67.
1913. A new *Alpinia* from Borneo. *J. Bot. Lond.* **51**, 247.
1913. *Muehlenbergia arundinella*, Ridl. n. sp. [in Decades Kewenses]. *Kew Bull.* p. 267.
1913. *Deschampia Klossii*, Ridl. n. sp. [in Decades Kewenses]. *Kew Bull.* p. 268.
1913. An expedition to Mount Menuang Gasing, Selangor; with an account of the journey by C. B. Kloss. *J. Linn. Soc.* (*Bot.*), **41**, 285.
1914. *Cypripedium pereirae*. *Gdnrs' Chron.* (3), **55**, 326.
1914. On a collection of plants from Gunong Mengkuang Lebah, Selangor. *J. F.M.S. Mus.* **5**, 28.

1914. Two new orchids from the Province of Bandon, S.W. Siam. *J. F.M.S. Mus.* **5**, 156.
1914. The plants from Koh Samui and Koh Pennan. *J. F.M.S. Mus.* **5**, 158.
1914. *Myrtus taxifolia*, Ridl., *Rhododendron andersonii*, Ridl. [in Decades Kewensis]. *Kew Bull.* p. 209.
1914. *Sarcopodium suberectum*, Ridl. [in New Orchids: Decade 42]. *Kew Bull.* p. 211.
1915. Two new plants from Gunong Tampin, Negri Sembilan. *J. F.M.S. Mus.* **6**, 38.
1915. Plants from Gunong Kerbau, Perak. *J. F.M.S. Mus.* **6**, 43.
1915. Plants from Gunong Tahan, Pahang. *J. F.M.S. Mus.* **6**, 127.
1915. Family XCVI *bis* Cytinaceae. *J. Asiat. Soc. Beng.* **75**, 393.
1915. A new Begonia from Borneo. *Sarawak M.J.* **2**, 177.
1916. On endemism and the mutation theory. *Ann. Bot. Lond.* **30**, 551.
1916. The natural history of Kedah Peak: v-vi. Botany. *J. F.M.S. Mus.* **7**, 37, 121.
1916. *Xyris oreophila*, Ridl. n. sp. [*in* Natural History of Kedah Peak]. *J. F.M.S. Mus.* **7**, 121.
1916. Report on the botany of the Wollaston Expedition to Dutch New Guinea, 1912-13. *Trans. Linn. Soc. Lond.* (*Bot.*) (2), **9**, 1.
1916. *Drimys elongata*, Ridl., *Aglaia rubra*, Ridl., and *Eriolopha flagellaris*, Ridl. *Icon. Plant. Hooker.* **31**, t. 3051, 3052, 3067.
1917. *Lasianthus barbellatus*, a new species from Pulau Tiuman, Pahang. *J. Straits. Br. Asiat. Soc.* **77**, 187.
1917. Introduction to the botany of Mount Korinchi, *and* Spermatophyta and Pteridophyta [*in* Results of an Expedition to Korinchi Peak, Sumatra]. *J. F.M.S. Mus.* **8**, iv, 9, 13.
1919. Fern-allies and Characeae of Malay Peninsula. *J. Straits. Br. Asiat. Soc.* **80**, 139.
1919. Cultivated Arundinas. *Orchid Rev.* **27**, 46.
1920. *Plantago cynops* in Kent. *J. Bot. Lond.* **58**, 271.
1920. New Malayan plants. *J. Bot. Lond.* **58**, 147.
1920. On a collection of plants from Peninsular Siam. *J. F.M.S. Mus.* **10**, 65.
1920. Two new Siamese plants. *J. F.M.S. Mus.* **10**, 127.
1920. Flora of Klang Gates, Selangor. *J. F.M.S. Mus.* **10**, 247.
1921. Scitamineae and Orchideae from S. Annam. *J. Nat. Hist. Soc. Siam.*
1921. The Indo-Malayan species of *Jussiaea*. *J. Bot. Lond.* **59**, 257.
1922. *Sandoricum Koetjape* and *Dendrobium caninum*. *J. Bot. Lond.* **60**, 273.
1922. *Rigiolepis* and other Vacciniaceae of Borneo. *Kew Bull.* p. 106.
1922. Musaceae, Zingiberaceae, Marantaceae. [*in* Contributions to our knowledge of the flora of Papua]. *Proc. Roy. Soc. Qd.* **34**, 19.
1923. New Euphorbiaceae from the Malay Peninsula. *Kew Bull.* p. 360.
1923. A botanical excursion to Northern Sumatra. *J. Straits Br. Asiat. Soc.* **87**, 46.
1924. *Garcinia holttumi*, Ridl. n. sp., *Grewia latistipulata*, Ridl. n. sp. *Microtropis peduncularis*, Ridl. n. sp., *Selanginella rivalis*, Ridl. n. sp. [*in* Decades Kewenses]. *Kew Bull.* pp. 261-66.
1924. *Bulbophyllum caesariatum*, Ridl. n. sp., *Eria citrium*, Ridl. n. sp. [*in* New Orchids: Decas L.]. *Kew Bull.* p. 202-4.
1924. *Lotus siliquosus* L. in Berks. *J. Bot. Lond.* **62**, 246.
1924. New Malayan plants. *J. Bot. Lond.* **62**, 294.
1924. *Evodia quercifolia* Ridl. n. sp. *Gdnrs' Chron.* (3), **76**, 303.
1925. New *Diospyros* from Sarawak [*Diospyros piscicapa*, Ridl. n. sp.]. *J. Bot. Lond.*, **63**, 51.
1925. Monocotyledones. [*in* Dr H. O. Forbes's Malayan Plants]. *J. Bot. Lond.* **63**, Suppl., 115.
1925. Endemic plants. *J. Bot. Lond.* **63**, 182.
1925. *Globba burbidgei*, Ridl. n. sp. (Borneo) [*in* Notes from the British Museum Herbarium]. *J. Bot. Lond.* **63**, 204.
1925. Plants from Bencoolen, Sumatra, collected by Mr C. J. Brooks. *Kew Bull.* p. 76.
1925. *Argostemma involucrata* Hemsl. var. *sumatrana* Ridl. n. var. [*in* Plants from Bencoolen, Sumatra]. *Kew Bull.* p. 83.

1926. The flora of the Mentawi Islands [*in* Spoila Mentawiensia]. *Kew Bull.* p. 57.
1928. *Cotyledon umbilicus* as an epiphyte. *J. Bot. Lond.* p. 273.
1929. *Chisocheton rigidus*, Ridl. n. sp., *Pseudoeugenia tenuifolia*, Ridl. n. sp. *Hederopsis major*, Ridl. n. sp. [*in* Decades Kewenses]. *Kew Bull.* p. 122-4.
1929. New species from the Malay Peninsula and Borneo. *Kew Bull.* p. 254.
1930. Additions to the flora of Borneo. *Kew Bull.* p. 74, 364.
1930. Myrtaceae Malayenses. *J. Bot. Lond.* **68**, 10.
1930. Asiatic Species of *Pleomele*. *J. Bot. Lond.* **68**, 177.
1931. Additions to the flora of Borneo and other Malay Islands, II-III. *Kew Bull.* p. 33, 493.
1931. *Melanorrhoea pilosa*, Ridl. n. sp., *Terminalia curtisii*, Ridl. n. sp. [*in* Decades Kewenses]. *Kew Bull.* pp. 448, 449.
1931. *Taraktogenos calophylla*, Ridl. n. sp. *Icon. Plant. Hooker.* **32**, t. 3167.
1932. The genus *Urophyllum* in Malaya. *J. Bot. Lond.* **70**, 191, 219.
1933. *Didissandra parviflora*, Ridl. n. sp. and *Sonerila vuttneri* Ridl. n .sp. *Arch. Hydrobiol.* Suppl.-Band xi, 327 (1932).
1933. Contributions towards a flora of British North Borneo, III. *Kew Bull.* p. 487.
1934. Contributions towards a flora of British North Borneo, IV. *Kew Bull.* p. 119.
1934. Firmiana and Erythropsis. *Kew Bull.* p. 214.
1934. Some dispersal notes. *J. Bot. Lond.* **72**, 52.
1934. New Malayan Rubiaceae. *J. Bot. Lond.* **72**, 251, 271.
1934. *Dorycnium gracile* Jord. in Kent. *J. Bot. Lond.* **72**, 349.
1935. Malay violets. *J. Bot. Lond.* **73**, 13.
1935. Malayan species of *Juncus*. *J. Bot. Lond.* **73**, 342.
1936. Notes on Malay Flacourtiaceae. *J. Bot. Lond.* **74**, 221.
1937. New Asiatic Scitamineae. *J. Bot. Lond.* **75**, 202.
1937. Origin of the flora of the Malay Peninsula. *Blumea* Suppl. 1, p. 183.
1938. *Arum neglectum* (Towns.) Ridl. n. sp. *J. Bot. Lond.* **76**, 144.
1938. Two new names in Aglaia. *Kew Bull.* p. 215.
1938. Notes on *Xylocarpus*. *Kew Bull.* p. 288.
1938-46. Additions to the flora of Borneo and other Malay Islands, VI-XIX. *Kew Bull.* (1938), pp. 110, 173, 221, 275; (1939) p. 29; (1946) p. 31.
1938. Two unrecorded gentians [*in* Additions to the Flora of Borneo and other Malay Islands, XI]. *Kew Bull.* p. 29.
1939. Bracken seedlings in London. *J. Bot. Lond.* **77**, 219.

Zoological

1873. Mammals and the Coleoptera of Haileybury. *School Nat. Hist. Report.*
1875. *Sphinx convolvuli* at Cobham, Kent. *Entomologist*, p. 223.
1876. Hibernation of *Hemerobius*. *Entomologist*, p. 48.
1878. Coleoptera hunting in 1877. *Entomologist*, p. 2.
1878. *Acronicta alni* in Hereford. *Entomologist*, p. 230.
1879. On a new copepod of the genus *Doridicola*. *Ann. Mag. Nat. Hist.* (5), **4**, 458.
1880. Observations on larva of *Chrysopa*. *Entomologist*, p. 21.
1880. Some English *Collembola*. *Ent. mon. Mag.* p. 1.
1881. Thysanura of Canary Islands. *Ent. mon. Mag.* p. 14.
1881. *Degeeria pulchella*. Ridl. n. sp. *Ent. mon. Mag.* p. 270.
1882. *Coronella laevis* in Surrey. *Zoologist*, p. 425.
1886. (With S. O. Ridley.) Animal life in high latitudes. *Zoologist*, p. 1.
1890. On the habits of the Caringa (*Formica gracilepes* Gray). *J. Straits Br. Asiat. Soc.* **22**, 345. [*See also* 1891, **23**, 147.].
1890. Mosquito larvae in the pitchers of *Nepenthes*. *J. Straits Br. Asiat. Soc.* **22**, 340.
1890. Coco-nut beetles. *J. Straits Br. Asiat. Soc.* **22**, 429.

1890. Notes on the zoology of Fernando Noronha. *Trans. Linn. Soc. Lond.* (*Zool.*), **20**, 473.
1890. *Thysanura* and *Collembola* of Fernando Noronha. *Trans. Linn. Soc. Lond.*(*Zool.*), **20**, 556.
1890. Occurrence of a crocodile on Cocos Islands. *Nature, Lond.* **42**, 457.
1891. The Keringga. *J. Straits Br. Asiat. Soc.* **23**, 147.
1891. Frugivorous habits of the Tupaia. *J. Straits Br. Asiat. Soc.* **23**, 148.
1894. Stick insects destroying orchids. *J. Straits Br. Asiat. Soc.* **26**, 204.
1894. List of mammals recorded from Pahang. [*in* An Account of a Trip up the Pahang and other Rivers]. *J. Straits Br. Asiat. Soc.* **25**, 57.
1894. A large beetle caught in a pitcher of *Nepenthes. J. Straits Br. Asiat. Soc.* **25**, 172.
1894. The bird-dropping spider (*Ornithoscatoides*) in Johor. *J. Straits Br. Asiat. Soc.* **25**, 172.
1895. The mammals of the Malay Peninsula. *Natural Science*, **6**, 23, 89, 161.
1895. A strange pet. *Mag. Selbourne Soc.* p. 89.
1895. Habits of *Macropisthodon rhodomelas. Zoologist*, p. 112.
1897. Birds especially useful in destroying insects. *Agric. Bull. Malay Pen.* **7**, 148.
1898. Birds in the Botanic Gardens, Singapore. *J. Straits Br. Asiat. Soc.* **31**, 73.
1898. The white snake of the Selangor Caves. *J. Straits Br. Asiat. Soc.* **31**, 99.
1898. *Hybla puera* Cram. *J. Straits Br. Asiat. Soc.* **31**, 104.
1899. The habits of Malay reptiles. *J. Straits Br. Asiat. Soc.* **32**, 185.
1899. An insectivorous squirrel. *J. Straits Br. Asiat. Soc.* **32**, 217.
1900. Insect pests. *Agric. Bull. Malay Pen.* **9**, 252.
1901. *Calogramma festiva* Walk. *J. Straits Br. Asiat. Soc.* **35**, 82.
1901. Habits of the drongo; the short-eared owl in Singapore; the Sumatran rhinoceros. *J. Straits Br. Asiat. Soc.* **35**, 105.
1901. Rambong beetle (*Batocera octomaculata*). *J. Straits Br. Asiat. Soc.* **36**, 138.
1902. Malay tiger-beetles. *J. Straits Br. Asiat. Soc.* **38**, 129.
1902. A coffee-bean pest. *Agric. Bull. Str. Malay St.* **1**, 386.
1903. *Grunda sikkima*, a rambong pest. *Agric. Bull. Str. Malay St.* **2**, 69.
1903. The black cobra. *Agric. Bull. Str. Malay St.* **2**, 225.
1903. A Castilloa borer. *Agric. Bull. Str. Malay St.* **2**, 322.
1903. A Futumia pest, *Caprinia conchylalis. Agric. Bull. Str. Malay St.* **2**, 355.
1904. A castor-oil pest, *Orphiusa melicerte. Agric. Bull. Str. Malay St.* **3**, 22.
1905. Red coconut beetle. *Agric. Bull. Str. Malay St.* **4**, 272.
1905. Caterpillar attacking tobacco plants. *Agric. Bull. Str. Malay St.* **4**, 391.
1905. Habits of the Tupaia. *J. Straits Br. Asiat. Soc.* **45**, 279.
1906. The menagerie of the Botanic Gardens. *J. Straits Br. Asiat. Soc.* **46**, 133.
1906. Curious nesting place of *Simotes octolineatus. J. Straits. Br. Asiat. Soc.* **46**, 263.
1908. Bats in a bamboo. *J. Straits Br. Asiat. Soc.* **50**, 103.
1908. The crackling moth. *J. Straits Br. Asiat. Soc.* **50**, 109.
1910. A termite's nest with eight queens. *J. Straits Br. Asiat. Soc.* **54**, 107.
1910. A nest of *Termes malayanus* with many queens. *Agric. Bull. Str. Malay St.* **9**, 13.
1911. Spangle-scale [*Lecamium expansum* var. *metallium*] on sour-sop. *Agric. Bull. Str. Malay St.* **10**, 74.
1911. Birds and crops. *Agric. Bull. Str. Malay St.* **10**, 232.
1911. Notes on Chinese swine-culture. *Agric. Bull. Str. Malay St.* **10**, 148.
1916. The large Mias in the Zoological Gardens. *Field*, 16th July.
1926. The habits of the pangelin. *Ctry Life, Lond.* Jan.
1927. Voices of apes. *Lond. Illus. News*, 17th Sept.
1929. Elephant cemetries. *Times*, 5th August.
1926. White blackbird in Kew Gardens. *Ctry Life, Lond.* Jan.
1932. Tree-viper caterpillar. *Proc. R. ent. Soc. Lond.* **7**, 46.
1934. Notes on insects. *Proc. R. ent. Soc. Lond.* **9**, 634.
1937. Apes in captivity. *Times*, 1st Oct.
1937. Bantu caves white snakes. *Times*, 10th Nov.
1938. Nature's tree planters. *Tree Lover*, p. 196.

Agriculture

1891. Dr Burck's method of treatment of the coffee-leaf disease in Java. *Agric. Bull. Malay Pen.* **1**, 2.
1892. Gambir. *Agric. Bull. Malay Pen.* **2**, 20.
1893. Dye plants. *Agric. Bull. Malay Pen.* **3**, 44.
1893. Patchouli. *Agric. Bull. Malay Pen.* **3**, 52.
1893. Ngai camphor [Blumea balsamifera]. *Agric. Bull. Malay Pen.* **3**, 55.
1893. Fibres. *Agric. Bull. Malay Pen.* **3**, 56.
1895. Sago. *Agric. Bull. Malay Pen.* **4**, 62.
1895. Notes on soils and lalang-grass. *Agric. Bull. Malay Pen.* **4**, 73.
1895. Indigo cultivation in Singapore. *Agric. Bull. Malay Pen.* **4**, 87.
1895. Copper sulphate as a timber preservative. *Agric. Bull. Malay Pen.* **4**, 87.
1895. Vanilla [and] ipecacuanha. *Agric. Bull. Malay Pen.* **4**, 88.
1896. Report on diseases of the nutmeg and clove trees. *Agric. Bull. Malay Pen.* **5**, 91.
1897. Spices. *Agric. Bull. Malay Pen.* **6**, 97.
1897. Ramie (*Boehmeria nivea*). *Agric. Bull. Malay Pen.* **7**, 138.
1897. Notes on sugar cultivation. *Agric. Bull. Malay Pen.* **7**, 141.
1898. Precocious coconuts. *J. Straits Br. Asiat. Soc.* **31**, 103.
1902. Gutta rambong (*Ficus elastica*) in Malacca. *Agric. Bull. Str. Malay St.* **1**, 185.
1902. Gutta jelutong (*Dyera costulata*). *Agric. Bull. Str. Malay St.* **1**, 188.
1902. Volatile oils. *Agric. Bull. Str. Malay St.* **1**, 335.
1902. *Ficus elastica* from seed. *Agric. Bull. Str. Malay St.* **1**, 467.
1903. Reclaiming abandoned mining land. *Agric. Bull. Str. Malay St.* **2**, 63.
1903. *Barringtonia* seed as a sakai food. *Agric. Bull. Str. Malay St.* **2**, 165.
1903. Fencing of main trunk line, Federated Malay States Railways. *Agric. Bull. Str. Malay St.* **2**, 245.
1903. Turf and fodder grasses. *Agric. Bull. Str. Malay St.* **2**, 273.
1903. Cotton. *Agric. Bull. Str. Malay St.* **2**, 309.
1904. Pineapples. *Agric. Bull. Str. Malay St.* **3**, 1.
1904. Pineapple cultivation. *Agric. Bull. Str. Malay St.* **3**, 37.
1904. A coconut pest. *Agric. Bull. Str. Malay St.* **3**, 92.
1904. A caterpillar attacking pepper plants. *Agric. Bull. Str. Malay St.* **3**, 101.
1904. Fibre plants of the Malay Peninsula. *Agric. Bull. Str. Malay St.* **3**, 302, 405.
1904. Soy and bean cheese. *Agric. Bull. Str. Malay St.* **3**, 494.
1905. Fibres (*continued*). *Agric. Bull. Str. Malay St.* **4**, 6.
1905. *Glycine hispida. Agric. Bull. Str. Malay St.* **4**, 49.
1905. Fibre of *Melochia corchorifolia. Agric. Bull. Str. Malay St.* **4**, 268.
1907. The oil-palm. *Agric. Bull. Str. Malay St.* **6**, 37.
1907. Lalang as a paper material. *Agric. Bull. Str. Malay St.* **6**, 379.
1908. A disease of cloves. *Agric. Bull. Str. Malay St.* **7**, 263.
1908. Banana fibre. *Agric. Bull. Str. Malay St.* **7**, 265.
1909. Variation in the flower of the black pepper. *Agric. Bull. Str. Malay St.* **8**, 244.
1909. Improvements in the manufacture of gambir. *Agric. Bull. Str. Malay St.* **8**, 312.
1910. Tillage of soil. *Agric. Bull. Str. Malay St.* **9**, 80.
1910. Chilis as a catch-crop. *Agric. Bull. Str. Malay St.* **9**, 450.
1911. Legislation against the dissemination of pests. *Agric. Bull. Str. Malay St.* **10**, 1.
1911. The drought spell. *Agric. Bull. Str. Malay St.* **10**, 181.
1911. Birds and crops. *Agric. Bull. Str. Malay St.* **10**, 232.

Rubber Industry

1897. Rubber cultivation. *Agric. Bull. Malay Pen.* **7**, 132.
1902. Para rubber stumps. *Agric. Bull. Str. Malay St.* **1**, 552.

1903. The history of the introduction of para rubber into the Malay Peninsula. *Agric. Bull. Str. Malay St.* **2**, 2.
1903. Boring beetles in para rubber. *Agric. Bull. Str. Malay St.* **2**, 222.
1903. Manuring para rubber. *Agric. Bull. Str. Malay St.* **2**, 351.
1904. *Astychus Chrysochloris* attacking para rubber. *Agric. Bull. Str. Malay St.* **3**, 21.
1904. Knots on para rubber trees. *Agric. Bull. Str. Malay St.* **3**, 20.
1904. Rubber-planting in Southern India. *Agric. Bull. Str. Malay St.* **3**, 160.
1904. Tapping scars in old wood. *Agric. Bull. Str. Malay St.* **3**, 159.
1904. Layering rubber trees. *Agric. Bull. Str. Malay St.* **3**, 441.
1905. Note on a peculiar flow of latex in a *Hevea*. *Agric. Bull. Str. Malay St.* **4**, 49.
1905. Tapping in rubber trees. *Agric. Bull. Str. Malay St.* **4**, 226.
1905. Rubber in Sarawak. *Agric. Bull. Str. Malay St.* **4**, 365.
1905. The position of rubber among cultivated plants. *Agric. Bull. Str. Malay St.* **4**, 381.
1905. A bark fungus on para rubber. *Agric. Bull. Str. Malay St.* **4**, 423.
1905. (With R. DERRY) Report on the experimental tapping of para rubber trees in the Botanic Gardens, Singapore, for the year 1904. *Agric. Bull. Str. Malay St.* **4**, 424.
1905. Rubber pests. *Agric. Bull. Str. Malay St.* **4**, 457.
1906. Exporting seeds of para rubber. *Agric. Bull. Str. Malay St.* **5**, 1.
1906. A fungus (*Irpex*) attacking roots of para rubber. *Agric. Bull. Str. Malay St.* **5**, 64.
1906. Tapioca as a catch crop for rubber. *Agric. Bull. Str. Malay. St.* **5**, 133.
1906. The old rubber tree at Penang. *Agric. Bull. Str. Malay St.* **5**, 138.
1906. Synthetic rubber. *Agric. Bull. Str. Malay St.* **5**, 369.
1906. *Attacus atlas* and rubber. *Agric. Bull. Str. Malay St.* **5**, 401.
1906. (With R. DERRY.) The second annual report on the experimental tapping of para rubber trees in the Botanic Gardens, Singapore, for the year 1905. *Agric. Bull. Str. Malay St.* **5**, 439.
1907. Abnormalities in the stem of *Hevea*. *Agric. Bull. Str. Malay St.* **6**, 157.
1908. Two rubber pests. *Agric. Bull. Str. Malay St.* **7**, 89.
1908. Big rubber trees in the Botanic Gardens. *Agric. Bull. Str. Malay St.* **7**, 253, 351.
1908. Size of rubber-seed. *Agric. Bull. Str. Malay St.* **7**, 335.
1908. Germination of para-rubber seed. *Agric. Bull. Str. Malay St.* **7**, 333.
1909. Various notes [on rubber]. *Agric. Bull. Str. Malay St.* **8**, 134.
1909. A new fungus-pest on para rubber (*Diplodia, Hymenochaetae*). *Agric. Bull. Str. Malay St.* **8**, 310, 521, 570.
1909. Rubber cultivation in so-called peat soil. *Agric. Bull. Str. Malay St.* **8**, 468, 516.
1910. Historical notes on the rubber industry. *Agric. Bull. Str. Malay St.* **9**, 20.
1910. (With R. DERRY.) Third report on experimental tapping of para rubber trees in the Botanical Gardens, Singapore, for the year 1909. *Agric. Bull. Str. Malay St.* **9**, 236, 289.
1910. Another para-rubber fungus. *Agric. Bull. Str. Malay St.* **9**, 216.
1910. Two para rubber fungi. *Agric. Bull. Str. Malay St.* **9**, 461.
1911. Rubber smoking house. *Agric. Bull. Str. Malay St.* **10**, 64.
1911. Duration of vitality of para rubber seed. *Agric. Bull. Str. Malay St.* **10**, 379.
1916. Cultivation of rubber. *Times* Supplement.
1955. Evolution of the rubber industry. *Proc. Inst. Rubber Industry*, **12**, 114.

Geology and Mineralogy

1886. Fossil wood of chestnut at Erith. *J. Bot. Lond.* p. 253.
1890. Geology of Fernando Noronha, based on petrological notes by Thomas Davies, F.G.S. *J. Linn. Soc.* (*Bot.*), **27**, 86.
1891. Diamonds in the Malay Peninsula. *J. Straits Br. Asiat. Soc.* **24**, 166.
1891. The raised reefs of Fernando de Noronha. *Amer. J. Sci.* (3), **41**, 406.
1894. Earthquake in the Malay Peninsula. *J. Straits Br. Asiat. Soc.* **25**, 169.

1899. Appendix to 'Caves in the Malay Peninsula' Committee Report. *Rep. Brit. Ass.* (1898), p. 572.
1933. Rocks grooved by *Ficus* roots. *J. Bot. Lond.* **71**, 75.
1938. Cause of the disappearance of Cycadeoidea in the Cretaceous period. *Ann. Bot. Lond.* New ser. **2**, 809.

Ethnology

1891. Discovery of a stone implement in Singapore. *J. Straits. Br. Asiat. Soc.* **23**, 141.
1899. Golden flowers. *J. Straits Br. Asiat. Soc.* **32**, 214.
[*Note:* this is a folk tale about the flowers of *Pandanus laevis*, also the flowers of *Ficus miquelii*].
1911. Causes of the disappearance of races. *Straits Phil. Soc.* p. 143.
1926. Cause of hairlessness in man. *J. Trop. Med.* (*Hyg.*), p. 303.

Medical

1897. Malay materia medica. *J. Straits Medical Assn.* **5**, 122.
1898. The poisonous plants of the Malay Peninsula. *Agric. Bull. Malay Pen.* **8**, 199.
1906. Malay drugs. *Agric. Bull. Str. Malay St.* **5**, 193.
1907. Chinese anti-opium drugs. *Agric. Bull. Str. Malay St.* **6**, 45.
1909. Remedies for snake-bite. *Agric. Bull. Str. Malay St.* **8**, 323.
1910. Poisonous principles of *Rhus*. *Pharm. J.* **84**, (4th ser. **30**), 360.
1911. *Rengas* poisoning. *Malay Med. J.* **9**, 7 (part 2).
1924. Poisoning by bamboo hairs. *J. Trop. Med.* (*Hyg.*), p. 278.
1925. Use of iodine in beri-beri. *J. Trop. Med.* (*Hyg.*), p. 102.
1930. On the action of poison of the Upar tree (*Antiaris*) *J. Trop. Med.* (*Hyg.*), p. 1.
1932. *Derris elliptica. Pharm. J.* **129** (4th ser. **75**), 387.

Biographical

1894. The botanists of Penang. *J. Straits Br. Asiat. Soc.* **25**, 163.
1897. In memoriam—H. Vaughan Stevens. *J. Straits Br. Asiat. Soc.* **30**, xxi.
1901. In memoriam—Dr N. B. Dennys. *J. Straits Br. Asiat. Soc.* **35**, 106.
1905. Sir Hugh Low. *Agric. Bull. Str. Malay St.* **4**, 239.
1908. Death of Mr A. B. Stephens. *Agric. Bull. Str. Malay St.* **7**, 66.
1910. Obituary: A. D. Machado [and] J. B. Carruthers. *Agric. Bull. Str. Malay St.* **9**, 328.
1910. Obituary: Dr Melchior Treub [and] Dr W. Burck. *Agric. Bull. Str. Malay St.* **9**, 495.
1911. Rt Rev. George Frederick Hose, D.D., Bishop of Singapore & Sarawak, 1881-1908. *J. Straits Br. Asiat. Soc.* **57**, 3.
1927. Rev. W. E. Taylor, M.A. [*In* Obituaries]. *J. Bot. Lond.* **65**, 317.
1928. Obituary. Charles Curtis (1853-1928). *J. Bot. Lond.* **66**, 332.
1929. H. C. Robinson [*In* Obituaries]. *J. Bot. Lond.* **67**, 261.
1932. Obituary. Sir Everard Ferdinand Im Thurn. *J. Bot. Lond.* **70**, 341.
1933. George Ramage [*In* Obituaries]. *J. Bot. Lond.* **71**, 75.
1934. Obituary. J. D. Gimlette (1867-1934). *J. Bot. Lond.* **72**, 176.

Books

1912. Spices. London: Macmillan.
1922-25. The flora of the Malay Peninsula. Vols. 1-5. London: L. Reeve.
1930. The dispersal of plants throughout the world. London: L. Reeve.

H R Robinson

HAROLD ROPER ROBINSON

1889-1955

Harold Roper Robinson, physicist and, in later life, outstanding figure in university administration, was born at 36 Ainslie Street, Ulverston, in Lancashire, on 26 November 1889. Until the outbreak of war in 1914 his life, with the exception of brief holidays, was spent in his home county. He was a faithful Lancastrian, although affectionately aware of the weaknesses as well as of the virtues of his county brethren, as witness, for instance, his 'Lancashire men, almost as markedly as their Yorkshire neighbours, combine a great pride in their own plain-speaking with an even greater intolerance of plain-speaking in others', quoted here as a typical Robinson pronouncement.

His father, James Robinson, was a solicitor's managing clerk and, it may be remarked, his son, now a London solicitor, at one time practised at Ulverston. The Robinsons were an old local family: one of his forbears used to write dialect stories under the name of *Geordie* and his uncle Roper Robinson was a famous Rugby football player. Harold was the eldest of a family of five, having three brothers and one sister. As a boy he went first to the Wesleyan School and later to the Ulverston Victoria Secondary School (now Ulverston Grammar School). It was in 1908, when he was just on nineteen, that he went to the University of Manchester, with a Lancashire County Council Scholarship. Another undergraduate who entered at the same time was James Chadwick (now Sir James), the discoverer of the neutron, who was two years his junior. The two became close friends, Robinson, as the elder, taking such lead as his greater experience gave him. They had many interests in common besides an enthusiasm for physics, such as English and French classical literature and the history of science. Robinson was at this time much concerned with boxing and swimming, interests which Chadwick did not share.

Apparently in Robinson's first year the teaching in the Physics Department was not very well organized and the students learned more from their laboratory work, not too much supervised, and from their own reading than from lectures. However, in his second year he came into contact with Rutherford, who had taken over the course of lectures on electricity and magnetism. Rutherford conveyed to eager students a sense of the excitement of discovery: he spoke of his own early experiments with the enthusiasm that always seized him when he was talking about matters in which he was deeply interested and on which he had pondered much. These lectures made a profound and lasting impression on both Robinson and Chadwick and probably determined the course which their future careers were to take. From then on Rutherford was the master influence in Robinson's scientific life: even after his death in 1937 Rutherford was, for Robinson, very much a living force.

Robinson took his degree, with first class honours in physics, in 1911, but in his third year, while he was still an undergraduate, he was selected by Rutherford to begin research under him on the heating effect of radium. Already in 1911 Robinson's name had appeared with that of Walter Makower, an active member of the staff of the department, A. J. Makower and W. M. Gregory on a little paper concerning the electrical state of the atmosphere, his modest research debut, but the first paper with Rutherford came out in 1912. It dealt with the development of heat by radium and radium emanation and was published first in Vienna, by the Akademie der Wissenschaften there, and the year after in an English translation. Those were busy days, in a laboratory throbbing with life. Robinson carried out with Moseley a piece of work on the total ionization produced by the β-rays and γ-rays of radium B and radium C, an early measurement on a subject teeming with difficulties, and with Rutherford an accurate determination of the magnetic deflexion of the α-rays from radium C, to give the mass and the velocity. But the most important work was undoubtedly that which he did with Rutherford on the magnetic spectrum of the β-rays from radium B and radium C. The method, in which the rays are deflected through a semicircle to produce a focusing effect on a photographic plate set parallel to the magnetic field, was much superior to those used earlier and has found wide application. This disposition had actually been already used by J. Danysz (*Le Radium*, **10**, 4, 1913), but it was the work of Rutherford and Robinson that developed it and brought it into prominence.

The research in question, which appeared in October 1913, gave the first accurate and extensive measurements of the complex β-ray groups of lines and paved the way for much further work. In August 1914, the month of the outbreak of the First World War, appeared two papers, one on *The magnetic spectrum of the β-rays excited in metals by soft X-rays*, by Robinson & W. F. Rawlinson, and one on *Spectrum of the β-rays excited by γ-rays*, by Rutherford, Robinson & Rawlinson. They involved the application of the magnetic focusing method to β-rays excited by radiation and were both short, but they are important as the starting point of Robinson's future research, which concerned the magnetic spectroscopy of electrons excited by homogeneous X-rays.

It should be recorded that in 1912 he obtained his M.Sc. degree by research and in the same year was appointed Assistant Lecturer and Demonstrator in Physics. He was made Senior Assistant Lecturer in 1915. The degree of D.Sc. awarded by the University of Manchester in 1917 must have been for research done before February 1915, when he was commissioned as second lieutenant. The degree of Ph.D. he took at Cambridge, in 1924, a rare case of the Ph.D. being subsequent to the D.Sc.

The years spent on research under Rutherford from 1911 until the outbreak of war were probably, in many ways, the happiest of Robinson's life:

he always looked back on them with great pleasure. He gave a brilliant and personal picture of the Manchester laboratory in those days in his Rutherford Memorial Lecture to the Physical Society, delivered in 1942 as the first of the series. Geiger, for a period Research Assistant, Bohr, Moseley, Darwin, Marsden, Nuttall, J. A. Gray, R. W. Boyle, D. H. C. Florance and Chadwick, already mentioned, were among those with whom Robinson was in close contact. An outstanding figure to whom Robinson paid tribute in his Lecture was the head laboratory steward, William Kay. The research laboratory of those days, with unbounded enthusiasm, no thought for the financial future but abundant thought for getting quickly the experimental results, and a ready improvisation of apparatus, necessary when the total available funds for research amounted to about £400 a year, is a thing of the remote past, and no doubt the laboratory of today has immense advantages. But the old lab was good family fun.

It was in 1913 that I first met Robinson, an impressive figure about six foot four inches in height, deliberate in movement and precise in utterance. He had a certain shyness and awkwardness which soon melted in the flame of common interests. He had wide interests, which included books, sport and the local music halls, then rich in notable and highly characteristic talent. He had little interest for female companionship. He was already an inveterate pipe smoker, as he remained until the last year of his life, when failing health curtailed this indulgence. He affected strong, rough and strange tobaccos, which were inexpensive, including one called, as far as I remember, ‘half-dark nailrod’, nailrod being defined, I now find, in the *Oxford English Dictionary*, as ‘coarse dark tobacco in the form of a thin rod or stick’. Naval plug tobacco, wrapped round with spun-yarn, and a particularly vile tobacco, in the form of an unwrapped cake to which the label was gummed direct on one side, were also among his pipe fodder. He was completely individual and if I had never met him since those days I should still remember him well.

In February 1915 he obtained a commission as second lieutenant in the Royal Garrison Artillery, and in August that year went to France, where he was actively engaged in sound-ranging, that is, locating German guns by measurement of the relative time of arrival of the sound of discharge at carefully surveyed points. Towards the end of his active service he was promoted Captain and acted as Adjutant of the 1st Field Survey Battalion, R.E., to which he had been attached while sound-ranging. After the war he returned to the University of Manchester as lecturer. Rutherford, who was still there at the time, wrote to Geiger on 14 June 1919: ‘Robinson is gradually regaining his interest in pure science again, but it takes time to get into the old swing.’ In the summer of 1919 Rutherford went to Cambridge as Cavendish Professor, being succeeded by Professor (now Sir Lawrence) Bragg. Robinson remained at Manchester and in 1920 was appointed Assistant Director of the Physical Laboratory

In this year he married Marjorie Eve Powell, a lecturer in economics in the University. It was a most happy match. In many ways husband and wife were a great contrast. Firstly in size, for she was a small woman: it was reported that their baby carriage had to have the handle bar adjustable for height. Again, she was bright, alert, sparkling and vivacious, while Robinson, though full of a dry humour, was deliberate and composed. They fully understood each other's temperament and made a charming pair, notably as host and hostess. Each took a loving, amused, almost tolerant delight in the other's difference. Until her death in 1939 they lived in warm understanding, affection and harmony, with much gaiety mingled with gravity when necessary.

In 1921 Robinson was elected to the Moseley Research Studentship, which had just then been founded to commemorate the brilliant young investigator, and with it went to work in the Cavendish Laboratory at Cambridge, thus resuming his intimate contact with Rutherford. Apparently he had not had opportunities for research at Manchester, for at the end of his paper giving the results of his work at Cambridge (*Proc. Roy. Soc.* A, **104**, 455, 1923) he expresses his thanks 'to the Council of the Royal Society for the grant of the Moseley Studentship, which has made it possible for me to resume scientific work'. He adds: 'It has been a source of pleasure to me that the work undertaken has been in that branch of physical science which originated in Moseley's classical experiments on the high-frequency spectra of the elements.'

This Cambridge investigation was a continuation of the work, begun with Rawlinson at Manchester, on the characteristic energies of the electrons released from metals by X-rays of known frequency: it was carried out with great improvements in the method, which led to increased accuracy, and with a large range of metals. The intensities were also estimated. The results were very valuable, for by measuring the line spectrum of the secondary corpuscular radiation Robinson was able to throw much light on the energy levels within the L, M and N groups in the atom. A typical remark that he made later in life was that he was not a Cambridge man, but had been an outpatient there for a year or two.

In 1923, after the two years at Cambridge in which this work was completed, he went to Edinburgh, where Barkla was then professor, as Reader in Experimental Physics and Carnegie Research Fellow. This was a newly instituted appointment, at £700 per year, probably equivalent there and then to £2500 or more today, half being paid by the University of Edinburgh for lecturing and the other half by the Carnegie Trust for research. His research was a continuation of the investigations of the electronic radiation produced by the incidence of homogeneous X-rays on elements. The results were described in two papers, the later of which, by himself and A. M. Cassie, appeared in the *Proceedings of the Royal Society.* The measured energies and intensities threw much light on the different sub-groups of electrons within the atom and on the conversion of radiation. In this paper with Cassie he wrote as his initials 'H.R.', as he always did hereafter, but before 1925 he used simply 'H'. He was christened Harold Robinson and must have added the family name of Roper about this time. The cause of the change is unknown to me.

In 1926 Robinson was appointed professor of physics at Cardiff, the full title of the institution being the University College of South Wales and Monmouthshire, Cardiff, a constituent College of the University of Wales. He succeeded Professor A. L. Selby. By this time Robinson, with his wide experience, wide interests, wide reading, diverse friendships, human sympathy and strong personality was a notable character, who quickly made his influence felt, first in his department and then throughout the College. From one somewhat shy in speaking before an audience, he had developed into an excellent lecturer who possessed the power of imparting to his students some of his own deep-seated enthusiasm for physics. He proved to be an excellent administrator, in spite of, or perhaps because of, his lack of formality. During his first two years at Cardiff much of his time was occupied in devising equipment for the new physics department, the erection of which was well on the way towards completion at the time of his appointment. In particular he gave much attention to the selection of new experimental apparatus of all kinds and finally, towards the end of 1928, to the organization of the transfer of his department from the Old College in Newport Road to the new establishment in Cathays Park.

He also spent much time on the preparation and delivery of lectures, for he made a point of giving at least one series in each of the Intermediate, Subsidiary, Final and Honours courses. He published a paper discussing the lines due to multiple ionization in the X-ray levels, sometimes called 'spark' lines, by analogy from optical spectra, and in due course continued, with C. L. Young, the experimental work on the magnetic spectrography of expelled electrons which had begun at Manchester, the results being published in 1930, the year in which he left Cardiff.

Robinson made an abiding impression at Cardiff. He endeared himself to the students by his accessibility, his lucid lectures and his humour, which sometimes evinced itself as a kind of grave fun. Shortly after his appointment, being in the chair at a popular lecture, he was asked to make some remarks. He deliberately rose very slowly to his great height, put one foot on his seat and continued to rise and then put the other foot on the desk in front of him, still rising until he stood with both feet on the desk and with his head some nine feet from the floor, concluding the performance with a beaming smile, and following it with a racy and characteristic speech. In the research laboratory he was a stimulating influence by reason of, among other things, his experimental skill. He was, for instance, very good at tracking down leaks in evacuated systems, which was a most necessary art at that date in the kind of work with which he was concerned. His obvious good will, enhanced by an occasional amusing cynicism, made him popular with his colleagues and he had sound judgment and other qualities which those in administrative control appreciated. In 1929 he was elected to the Fellowship of the Royal Society and by happy chance it was Rutherford, being then President, who admitted him. One of his last public duties at Cardiff was to take part in the formal opening of the Tatem Laboratories by the then Prince of Wales, later Edward VIII.

It was at the end of the academic year in 1930 that Robinson left Cardiff to succeed Professor C. H. Lees, who had occupied the post since 1906, as Professor of Physics at what was then East London College, now Queen Mary College, University of London. Lees, incidentally, died only three years before Robinson. Here, again, certain difficulties awaited him. The old physics department was an extremely inconvenient conglomeration of rooms from which it was difficult to develop a well-planned department. Certain rooms were added as time went on, but he was never able to obtain what he had hoped for, a well-planned new department such as he had had at Cardiff.

His experience and his personality soon made him an outstanding figure in the administration of the College, of which he was elected Fellow in 1935. Fellowships in Colleges of the University of London are, of course, marks of approval unsullied by the financial association that pertains in certain older institutions, and therefore very highly esteemed. Robinson was, of course, also Professor in the University of London, and had to fulfil many duties in the way of attending boards, faculties, committees and other corporate bodies, and of taking an active part in both internal and external examinations. In all administrative matters his balance and the confidence which he inspired in both his colleagues and the university authorities soon brought him into prominence. He took an active interest in the teaching of the history of science and besides being a member of the University Board of Studies in Physics, a duty which his university professorship involved, he was from 1937 to 1953 a member of the Board of Studies in the History and Philosophy of Science.

In 1936 the University of London appointed Robinson as its representative on the governing bodies, or boards, of the Old Vic and Sadler's Wells, institutions which are two separate foundations actively concerned with the presentation, without profit, of classical plays, and of operas and ballets, respectively. This represented a new and lively interest in his life and he speedily made his influence felt in what would generally have been thought strange surroundings for a university professor of physics. In 1937 he was chosen as Chairman of the Vic-Wells Association, which is a society whose members are recruited from the regular audiences at the performances staged by the two foundations. Here his help was so much appreciated that he retained the position intil 1947, when he resigned owing to pressure of work: as a recognition of his services he was elected Vice-President in 1949. When in 1940 he had to leave London owing to the emigration of Queen Mary College he found it necessary to resign from the boards of the Old Vic and Sadler's Wells, but in 1949 he was re-appointed to the governing body of the Old Vic and two years later he became a governor of the Sadler's Wells School, where a general education combined with training in the ballet is provided for young people. He thus took a very keen and helpful part for over twenty years in the direction of these two bodies that have done, and do, so much for the English theatre. When Robinson died the Vic-Wells Association recorded 'His courage and integrity commanded our admiration and his wisdom, humour and kindliness endeared him to us all', a very justly worded tribute.

At Queen Mary College Robinson resumed experimental work on the measurement of the magnetic spectrum of the electrons expelled from metals by homogeneous X-rays, with a view to obtaining the greatest possible precision, so that accurate values of the atomic constants might be deduced. In 1932 he discussed the question of the best values of e, m_0 and h, comparing in particular the values of X-ray terms deduced from crystal diffraction with those deduced from his measurements on expelled electrons. He followed this with a paper describing new experiments, carried out in collaboration with J. P. Andrews and E. J. Irons, on the electrons expelled from certain metals by homogeneous X-rays from copper, renewing in their light the discussion of atomic constants. This was a subject to which he often returned, making, with Clews, further measurements on expelled electrons and discussing at length the accurate values of X-ray levels and the derived values of atomic constants. In 1935 he wrote a considered report on the value of the charge of the electron, which he continued in 1937, concluding 'There is much reason for thinking that the value $e=4\cdot803\times10^{-10}$ e.s.u. may be correct to within 1 part in 10000', which accords with the value accepted today. His last scientific paper, a short one published in 1940, describes further measurements, made with Clews, on the magnetic spectrum of electrons expelled from metals by a homogeneous silver X-ray radiation.

On the outbreak of war in 1939 Queen Mary College removed to Cambridge, where its members were the guests of King's College. Here Robinson was active in taking combined classes in electricity of Queen Mary and Cambridge men for Part I of the Natural Science Tripos and in other teaching work. His wife had not been well for some time and at Cambridge a cruel and deadly disease manifested itself. She died there on 6 December 1939. Her death and the manner of it was a heart-breaking blow for Robinson, for he was a man of deep feeling and tenderness and between them was a profound love. The wound left abiding scars.

At the end of 1940 he married Madeleine Symons, who had been an intimate friend of his first wife and possessed the trust of his two children, a confidence which soon ripened into something more. She had a very pleasant country abode, adapted from roomy old cottages, near Chipping Campden, where they were able to relax in vacation time, often in the company of friends whom they generously invited to share their quiet.

At the end of the war Queen Mary College returned to London in time for the beginning of the 1945-46 session, and the Robinsons went to live in a house in South Kensington, which became a charming home. Here his second wife created a serene atmosphere in which Robinson built up his composure and active spirit of service again. She was well known in political circles and very active in disinterested social activities, being especially concerned with juvenile delinquency, on which she was an authority. Robinson's activities in University administration and his interest in the creative theatre had her full sympathy.

At Queen Mary College there were great difficulties in starting and building up again the physics department. The buildings had suffered much during their use for emergency purposes, necessitated by the conditions in wartime London, so that considerable repairs were necessary to fit them for university work again. The department was short of staff to deal adequately with the influx of new and strange students and it was hard to obtain apparatus which was needed. The circumstances were not favourable for the resumption of research work and actually Robinson never undertook any new investigations. He became much involved in College administration and was made Vice-Principal in 1946, an arduous office which he continued to hold, with one year as Acting Principal from 1951-52, until 1953, when the deterioration of his health led him to resign his University chair, at the age of sixty-three. He was made Emeritus Professor, a title which carries no privileges.

After the second world war Robinson served on several committees of the Royal Society, including the Library Committee, the Newton Letters Committee and the Rutherford Memorial Committee. During the war he had been a member of the Sectional Committee for Physics. He had joined the Physical Society on coming to London in 1930 and served on the Council for three periods, the last being 1944-46. He took an active interest in the Society and was a Vice-President for the years 1946-48, so that the Royal Society and the Physical Society must be counted among the concerns of his last ten years.

In this period he became very much interested in the administrative side of the University of London. In 1945 he was appointed to the Senate as representative of the University Faculty of Science, a body which he attended. In the same year he became a member of the Academic Council. In 1946 he was appointed to the Central Research Fund Committee, in 1947 to the Professoriate Committee and in 1951 to the Honorary Degrees Committee. These are examples of his administrative activities. Such was his position and his reputation that in March 1954, when the then reigning Vice-Chancellor, Sir Roderick Hill, became gravely ill, he was made Deputy Vice-Chancellor, although he had already retired from his University Chair. Sir Roderick resigned in June 1954 and Robinson then became Vice-Chancellor of the University, with the privileges and membership of University bodies that pertain to the position. Throughout his work for University administration he behaved as a statesman rather than a politician, which is not invariably the course of conduct of Vice-Chancellors.

The work of a Vice-Chancellor, while interesting, useful and honourable, is hard and exacting, and Robinson's health unfortunately continued to deteriorate in such a way that in October 1955 he found it necessary to resign the office. While it was clear that he was in no state to cope with arduous public duties it seemed to those close to him that many years of quiet life with his family, his friends and his books might well be before him. However, it was not to be. On 28 November, two days after his sixty-sixth birthday and a month or so after his retirement from the Vice-Chancellorship, he died quietly and quite unexpectedly in his chair.

His death caused a very real and widespread distress in the many widely different circles where he was known, as well as among his intimates. There were many tributes to his life and personality, including a very fully attended memorial service held at St Martin-in-the-Fields, at which Lord Adrian, at the time President of the Royal Society, read the lesson and Dr J. F. Lockwood, Vice-Chancellor of the University of London, read the passage from *The Pilgrim's Progress* recording how Christian passed over the River.

His wife survived him, but not for long, for she died on 21 March, 1957.

Robinson, apparently but deceptively a simple character, was not always understood. He had an outward manner that was liable to mislead those who did not know him into thinking that he was in some matters less serious, in others more assured, than he actually was. After he matured, for instance, he sometimes affected to be a man who took things easily, who was unwilling to exert himself unduly. Actually he was an extremely hard worker who, if he thought that the matter in hand called for it, drove himself unmercifully. He was generally held, on account of his casual utterances, to be strongly against getting up early in the morning, and I presume that he, like many others, sometimes felt that another half hour in bed would be pleasant, but I am assured on excellent authority that when the time-table of college courses was revised at Cardiff he succeeded in getting the time of certain lectures, which he himself delivered, put back to 9 a.m., saying that he liked 9 o'clock lectures. The lenient judgments to which he occasionally gave voice in the presence of those not completely in his confidence were often at variance with the caustic comment which he would utter in congenial company. For instance, at a time when a certain new discovery was under discussion, he said, 'I was just beginning to think that there might be something in it when I heard that John Doe had confirmed the result', 'John Doe' being a very well-known physicist. In general, he was tender-hearted rather than tough, when trouble or cruelty of any kind was concerned, although not lenient to laziness or to the pretentious incompetence which is sometimes found even in scientific circles. He liked many people but cared deeply for a few only.

He had one great hero and that was Rutherford. One who knew him well writes to me that 'Rutherford had so impressed Robinson with his genius that the latter appeared to be unable to see greatness in any other of his contemporaries', which I quote not as showing that Robinson had a lack of appreciation of others, but to indicate how strongly his devotion to Rutherford impressed those who knew him. Rutherford was a towering genius and a man with an overmastering enthusiasm which he never attempted to restrain: he was, perhaps, at his most creative, certainly his most untrammelled, during the period when Robinson was closest to him: Robinson at that time was in the early twenties, one of the most impressionable periods of a man's life. This devotion is, then, scarcely surprising. Their temperaments, too, fitted well together: Rutherford's brusque, excitable ways, blunt speaking and sometimes hasty judgments never ruffled the calm, generally smiling, slow-spoken Robinson, who towered above him physically. Rutherford undoubtedly had a great liking and understanding for Robinson, who was probably on terms as intimate with him as were any of the number who worked in his laboratory at different times and in different Universities. Robinson's lecture on 'Rutherford: life and work to the year 1919 . . .', to which reference has already been made, gives the best picture extant of the man at the period.

This influence of Rutherford's is clear in Robinson's original scientific work after he left Manchester. This, as has been indicated, lay entirely in one region, investigations of the magnetic spectrum of the electrons liberated by homogeneous X-rays, and thus originated in work done in the Manchester laboratory just before the first World War. Robinson's experimental work was of the highest quality; his discussion of the results precise and penetrating; but he never strayed from this somewhat narrow field.

Robinson was a man of wide reading, in French as well as in English, and by no means entirely in conventional fields. He had an excellent memory and a keen critical sense and was a most entertaining talker. He would also have liked it mentioned that he had a pretty taste in wine. He was a keen collector of old books on scientific subjects and a discriminating buyer who, especially in the period between the wars, picked up many bargains—in fact, he seldom bought unless the price was advantageously low. Correspondingly his knowledge of the history of physical science was extensive and, in certain fields, profound. Unfortunately, he never wrote anything on the subject. But, except for papers describing his researches, he wrote little, which is much to be deplored.

Robinson belonged to a generation that grew up in a period when physics was a glorious adventure and not an overadministered but underpaid profession, when professors spent their time in the pursuit of learning which they did not expect to corner in the committee room. The youth of this generation was cut short in full bloom by the first world war, its middle age blighted by the second world war, its later life automatically excluded from the laboratory and lecture platform where its fathers found comfort in their later summer and autumn. Robinson bore all his lesser troubles with a good-natured tolerance, and the great misfortune of the loss of his first wife, which for a time numbed his life, with fortitude. He was a man warmly loved and respected both within the academic courts and outside those walls, singularly free from priggishness of all kinds. It says much for one of such strong personality that he had no real enemies and that he was trusted in all councils in which he took part. Take him for all in all, he was a good, a learned, a humorous and a both humane and human man, who applied his great powers wholeheartedly and unselfishly to whatever, in physics or in wider walks of life, he undertook. He will long be remembered by all who knew him.

Robinson left behind him no notes or records of his career, so that I have necessarily had recourse to many of those who knew him for help concerning different phases of his career. For information which I have used in this notice my particular thanks are due to Sir Norman Birkett, Sir James Chadwick, F.R.S., Professor R. T. Dunbar, Mr G. F. H. Harker, Dr E. J. Irons and Miss Annette Prevost, as well as to officials of the Universities of London and Manchester.

E. N. da C. Andrade

BIBLIOGRAPHY

1911. (With A. J. Makower, W. Makower and W. M. Gregory.) Investigations of the electrical state of the upper atmosphere made at Ditcham Park, Petersfield, August 1910. *Quart. J. R. Met. Soc.* **37**, 341-349.

1912. (With E. Rutherford.) Wärmeentwicklung durch Radium und Radiumemanation. *S.B. Akad. Wiss. Wien*, **121**, Abt. IIa, 1491-1516. (Also printed in English, *Phil. Mag.* **25**, 312-330, 1913.)

1913. (With E. Rutherford.) The analysis of the β-rays from radium B and radium C. *Phil. Mag.* **26**, 717-729.

1913. (With E. Rutherford.) Über die Masse und die Geschwindigkeiten der von den radioaktiven Substanzen ausgesendeten α-Teilchen. *S.B. Akad. Wiss. Wien*, **122**, Abt. IIa, 1855-1884. (Also printed in English, *Phil. Mag.* **28**, 552-572, 1914.)

1914. (With W. F. Rawlinson.) The magnetic spectrum of the β rays excited in metals by soft X rays. *Phil. Mag.* **28**, 277-281.

1914. (With E. Rutherford and W. F. Rawlinson.) Spectrum of the β rays excited by γ rays. *Phil. Mag.* **28**, 281-286.

1914. (With H. G. J. Moseley.) The number of ions produced by the β and γ radiations from radium. *Phil. Mag.* **28**, 327-337.

1923. The secondary corpuscular rays produced by homogeneous X-rays. *Proc. Roy. Soc.* A, **104**, 455-479.

1925. The K absorption levels of the light atoms: a correction. *Nature, Lond.* **115**, 945.

1925. X-ray terms and intensities. *Phil. Mag.* **50**, 241-250.

1926. X-rays—internal absorption and 'spark' lines. *Nature, Lond.* **118**, 224.

1926. (With A. M. Cassie.) The secondary and tertiary cathode rays produced by external and internal absorption of homogeneous X-rays. *Proc. Roy. Soc.* A, **113**, 282-301.

1927. Multiple ionization in X-ray levels. *Phil. Mag.* **4**, 763-774.

1929. (With C. L. Young.) The absorption of X-rays. *Nature, Lond.* **123**, 203.

1930. (With C. L. Young.) The influence of chemical state on critical X-ray absorption frequencies. *Phil. Mag.* **10**, 71-75.

1930. (With C. L. Young.) New results of the magnetic spectroscopy of X-ray electrons. *Proc. Roy. Soc.* A, **128**, 92-114.

1932. Numerical values of atomic constants and X-ray terms. *Phil. Mag.* **14**, 605-612.

1933. (With J. P. Andrews and E. J. Irons.) Atomic constants deduced from secondary cathode ray measurements. *Proc. Roy. Soc.* A, **143**, 48-60.

1934. The photoelectrons expelled from elements by chromium K radiations. *Proc. Phys. Soc.* **46**, 693-702.

1934. Atomic constants deduced from secondary cathode ray measurements. *Nature, Lond.* **134**, 179.

1934. X-ray levels and atomic constants. *Phil. Mag.* **18**, 1086-1099.

1935. (With C. J. B. Clews.) The secondary cathode rays expelled from metals by molybdenum K α radiations. *Proc. Roy. Soc.* A, **149**, 587-595.

1935. Auger effect and forbidden transitions. *Nature, Lond.* **135**, 826.

1935. The charge of the electron. *Phys. Soc. Rep. Prog. Phys.* **2**, 247-257.

1936. X-ray levels and atomic constants. *Phil. Mag.* **22**, 1129-1136.

1938. The charge of the electron. *Phys. Soc. Rep. Prog. Phys.* **4**, 212-217.

1938. Viscosity of air and the electronic charge. *Nature, Lond.* **142**, 159.

1939. (With R. L. Mayo.) Auger and secondary X-ray electrons from gold. *Proc. Roy. Soc.* A, **173**, 192-200.

1940. (With C. J. B. Clews.) X-ray electrons expelled from metals by silver K α radiations. *Proc. Roy Soc.* A, **176**, 28-33.

1943. Rutherford: life and work to the year 1919, with personal reminiscences of the Manchester period. *Proc. Phys. Soc.* **55**, 161-182.

Henry Norris Russell

HENRY NORRIS RUSSELL

1877-1957

HENRY NORRIS RUSSELL was born on 25 October 1877 at Oyster Bay, New York. Like so many starred American scientists he was a son of the manse, his father being the Rev. Alexander G. Russell, born in Nova Scotia of Scottish stock. His mother was Eliza Norris, who came of a line of New England sea captains; she was born at Para in Brazil where her father was temporarily United States consul. Russell came to Princeton, the home of his mother's family, at the age of twelve. He graduated A.B. at Princeton University at the age of nineteen *insigne cum laude*, 'the only time in recent Princeton history that this ultramaximum honour has been granted'.

Working under A. C. Young in astronomy he graduated Ph.D. in 1900. He had already published several papers (on a new graphical method of determining the elements of a binary star orbit, on the densities of variable stars of the Algol type, on the elements of Planet DQ and of Eros, on measurements of the diameter of Jupiter, and on perturbations of the major axis of Eros by the action of Mars) before he proceeded in 1902 to King's College, Cambridge as, what was called in those days, an advanced student. In the following year he worked with A. R. Hinks at the Cambridge Observatory as a Research Assistant of the Carnegie Institution of Washington, using the Sheepshanks telescope on photographic parallaxes of stars. A serious illness prevented him from carrying out the full observational programme which was completed by Hinks. The material was later forwarded to Russell at Princeton for measurement and reduction.

He returned to Princeton University in 1905 as an Instructor in Astronomy, becoming Assistant Professor in 1908, Professor in 1911, and Director of the Princeton University Observatory in 1912. In 1927 he was elected to the C. A. Young Research Professorship, which was endowed by his classmates at Princeton, the class of 1897. He retired from this chair and the Directorship of the Observatory in 1947. He was appointed a Research Associate of the Mount Wilson Observatory in 1921, of the Lick Observatory as Morrison Research Associate, and of the Harvard Observatory in 1947. He also regularly visited the Lowell Observatory, the National Bureau of Standards and other research groups. It is safe to say that no one in America has ever equalled his influence throughout fifty years as an inspiring consultant on problems and programmes of work in astronomy, astrophysics and spectroscopy. The assistance he gave by way of criticisms, hunches and computations was immense. He contributed researches of a fundamental nature to so many branches of astronomy that it is necessary to divide an account of his work into separate sections, indicating here and there where the different lines of research interlock.

I. PLANETS

As some of his earliest papers refer to members of the solar system these will be first discussed. In an examination of the cusps of Venus he observed that the sunlit and visible arcs measured along the limb extended 1°10′ further than they would in the case of an opaque sphere without atmosphere; he deduced from the twilight illumination of the elongation that the atmosphere of Venus was more than one-third as dense and extensive as the earth's atmosphere. In 1907 with Z. Daniel he observed Venus as a luminous ring under conditions which will not now recur until 1972, the planet being 1°49′ from the centre of the sun's disk; the twilight arc was measured as 55′.

A careful study of the stellar magnitudes and colour indices of the sun, moon, planets, satellites and a few asteroids led to a discussion of the albedo of planets and their satellites and of the particles of Saturn's rings, also of the probable causes of light variation in the case of asteroids. Russell gave $-25^{m}\cdot 93$ as the photographic magnitude of the sun with a colour index $+0^{m}\cdot 79$, and for the full moon $-11^{m}\cdot 37$ and $+1^{m}\cdot 18$ respectively. He estimated the stellar magnitude of the earth as seen from the sun as $-3^{m}\cdot 5$.

In an examination of the theory of capture by planets of the periodic comets Russell showed that Jupiter must be responsible for almost all the captures and he gave a number of general conclusions such as the need for the orbit of any captured comet to pass close to the orbit of the planet which last captured it. He suggested that two groups of comets with periods about 406 and 775 years might be families due to undiscovered remote planets. On the whole there was a good fit between observations and theory.

Lastly mention must be made of a series of papers on the determination of the position of the moon by photography, a study undertaken after discussion with E. W. Brown and E. C. Pickering. The plates were all secured by E. S. King at Harvard, measured and reduced (according to plans prepared by Russell) by Russell and a succession of assistants. An occulting disk in front of the object glass prevented the light from the moon from reaching the plate, except when the disk was turned edgewise on for a fraction of a second. Light from stars a little distance from the moon passed by the edges of the disk. The chief difficulty in the reduction lay in the limb correction for which Hayn's values were used. It was claimed that the photographic method gave results somewhat superior to meridian observations.

II. Parallax

Russell's photographic work on the moon described above was undertaken as a natural sequel to his work on photographic stellar parallaxes carried out at Cambridge and Princeton. In this work which was Russell's first major piece of research some of Russell's characteristics already show themselves—thus he paid close attention in his programme of reduction to the most advantageous relation between the labour employed and the results obtained. He noted too the wide range of absolute magnitudes and proper motions of different groups of the stars examined for parallax; from various explanations of the relation between brightness and proper motion of these groups he had no hesitation in deciding in favour of wide differences in luminosity as the principal factor involved; a first step towards his theory of stellar evolution came in his separation of the red stars into two groups, early and late, of high and low luminosity.

The direct determination of trigonometric parallaxes by photographic methods was followed by a study of mean parallaxes deduced from the parallactic motions of groups of more distant stars or from groups of stars with peculiar motions. For stars with proper motions less than 14 km/s Russell found that parallactic motions gave the best results; for stars with higher peculiar velocities he found that results of value could be obtained for rather small groups of stars.

Russell was one of a group of several astronomers (Comstock, Hertzsprung and Jackson) who developed methods of determining the parallaxes of binary stars, to which he gave the name 'dynamical parallaxes'. This method was closely tied up with the assumption that the mass of a component of a binary system was equal to the mean value for the mass of a star of the spectral class concerned. With Dr C. E. Moore he published a catalogue of 1777 dynamical parallaxes of double stars using Eddington's mass-luminosity law. Russell took an active part in discussing the relations between the various determinations of stellar parallax—trigonometric, parallactic, dynamical and spectroscopic. He also discussed formulae for the corrections to be applied to different sets of results to meet their systematic errors.

III. Binary systems

Russell's work on binary stars dates from his student days, his first published paper being on a new graphical method of determining the elements of a double-star orbit. He published no fewer than four papers on the calculation of the orbits of spectroscopic and visual binaries as well as much work (to be discussed below) on eclipsing variables. The emphasis was largely on the ease and rapidity of the method of calculation. He illustrated his methods by the determination of the orbits of a number of individual binaries.

A great deal of Russell's work on the calculation of stellar masses is summarized in his monograph on 'The masses of the stars' which in collaboration with Dr C. E. Moore he published in 1940 as a Harvard astrophysical monograph. It arose from a lecture at a Harvard centenary. He gave all the direct evidence available on the masses of stars derived by gravitational methods from binary systems. He stated that reliable values from visual binaries only existed for a couple of dozen systems while there were only just as many eclipsing variables spectroscopically observed. For the rest there were only average values of selected groups. Russell stated that divergences from the mass-luminosity law came from such groups as white dwarfs and the Trumpler stars. The statistical methods employed could be tested by the study of triple systems.

Russell found high values of mass-ratios in such systems in cases where the plane of the secondary orbit coincides approximately with that of the visual orbit. In a discussion of tidal action on the separation of the two components of a gaseous mass separated by fission he examined what happens when the smaller mass or both masses undergo a second fission. Comparing his theoretical results with observation he found in favour of fission as the best working hypothesis to adopt for the origin of binary and multiple stars—until some evidence either observational or theoretical should be produced to oppose it. Russell always kept his mind open to the need to revise his theories when fresh evidence came along.

IV. Eclipsing variables

Another subject which Russell fathered in astronomy and to which he made pioneering contributions from his student days right to the end was that of eclipsing variables. His first paper on the subject, published in 1899, dealt with the densities of the Algol variables. From a comparison of the period and the duration of the light-variation of a binary system he calculated that the stars were much less dense than the sun; probably the density was less than a quarter that of the sun, though the core might be more dense if there was an extensive outer atmosphere.

Later Russell developed the first general methods for the determination of the orbits of eclipsing variables and published tables to be used in obtaining the elements of the orbit. Starting with the simple case of two spherical stars and circular orbits he passed on to eccentric orbits, with accompanying asymmetry in the light-curve, and elliptic stars elongated by mutual attraction; then he allowed for light on a star's disk due to reflexion from its companion and for darkening at the limb. With Harlow Shapley he calculated tables allowing for darkening at the limb, developed fundamental equations and worked out directions for the use of those computing an orbit; he also carried out a number of computations himself (alone or with others) on individual stars. From a study of the distribution of the eclipsing variables with reference to the galaxy Russell, with Shapley, deduced the effect of interstellar absorption. The long series of monographs on individual eclipsing variables by his colleague, R. S. Dugan, and others bears witness to the stimulus of his general direction.

He studied 2111 observations of six eclipsing variables; he obtained sets of elements representing both the visual and photographic observations in all six cases, though the relative brightness of the two components and the degree of limb darkening were different for the two sets of observations. He noted that when the two components were of nearly equal brightness they were of the same colour, but if one component had a lower surface intensity it was the redder of the two: the whiter was generally the smaller and more dense.

In a couple of papers on the advance of periastron in Y *Cygni* Russell took into account the oblateness of the components due to rotation and the ellipticity due to tides: he fitted observations to theory, but failed to account for the magnitude of the actual apsidal motion. He returned to the subject of apsidal motion after Cowling had published his formula for a close binary and took into account darkening at the limb, and variation of surface brightness with gravity. He recommended that, when determining the geometrical elements of eclipsing variables, ellipticity of figure should be calculated on dynamical principles from stars of the Roche model type. An examination of a number of individual stars pointed to a high concentration of density in the cores of all short-period binaries, the central density being from 40 to 400 times the mean density.

In 'The royal road of eclipses' (the first Henry Norris Russell lecture delivered at Radcliffe College under the sponsorship of the American Astronomical Society in 1946) he gave a good account of the problem of determining the orbits of eclipsing binaries: in dealing with numerous refinements he referred to the work of Kopal. In determining orbits he stated in later papers that preliminary and intermediate elements should represent the complete light curve and should include ellipticity, reflexion, orbital eccentricity and limb darkening corresponding to the spectral type. Corrections for differences in shape and gravity effect could then be made by the use of Kopal's tables. Russell added a nomogram for use in partial eclipses. He emphasized that only a satisfactory system of intermediate elements justifies the heavy labour of further refinements.

Later observational developments, the increased accuracy of photoelectric cells, give results which go beyond the limits of graphical solutions. Russell made further suggestions for a rapid solution of the preliminary orbit and developed a fuller and final theory with J. E. Merrill, giving instructions for the use of a series of tables and nomograms prepared by Merrill; these are for corrections after a rectified light-curve has been used to obtain a set of preliminary elements. This paper was Russell's last major publication and it rounded off a life's work on eclipsing variables. His last paper, only published a week before his death was an article on eclipsing variables in the Magellanic Clouds, in *Vistas in Astronomy*. This gave a comparison of light-curves with those of galactic eclipsing variables. The mean radii of the components and mean densities were fairly well determined and are similar to those of the galactic variables. There are only a small number of them compared with the Cepheids.

V. Stellar evolution

As mentioned above Russell's early work on photographic parallaxes of stars had convinced him of the existence of two groups of stars of very different luminosities. Independently Hertzsprung had also found (*Zeitschrift für wissenschaftliche Photographie*, **5**, 86, 1907) two groups of stars of the same spectral class of very different brightnesses which he christened 'giants' and 'dwarfs', names adopted by Russell. The famous Hertzsprung-Russell diagram plots the absolute magnitudes of stars against their spectral types and shows the steady change of brightness along the main sequence and the existence of stars in the later spectral types far exceeding in brightness those of the same spectral type in the main sequence. This diagram was used as an illustration in an address in December 1913 to a joint meeting of section A of the American Association for the Advancement of Science and of the Astronomical and Astrophysical Society of America. It was published in May 1914 in *Popular Astronomy* and in *Nature*. Russell had already begun to relate the discoveries of giants and dwarfs to the general question of stellar evolution, returning to Norman Lockyer's general conception of the problem, and the theories of Homer Lane and Ritter on the evolution of a condensing mass of cool gas. Behind the criteria which Russell adopted lay a wealth of material gathered from his work on binary stars and clusters. Alfred Fowler in an address in 1921 on the presentation of the Gold Medal of the Royal Astronomical Society to Russell for his 'Contributions to stellar evolution' pointed out the great extent to which later work such as Adams's spectroscopic parallaxes and Pease's interferometer measurements of stellar diameters had supported Russell's views; as also had a better understanding of the physical meaning of detailed differences in the spectra of giant and dwarf stars.

In his many discussions of the changes with the progress of time in temperature, density, brightness, spectrum and other observable characteristics of a star Russell dwelt on their dependence on those factors which remained invariant for a system, such as mass, angular momentum, chemical composition. His main theory is summed up in his article on stellar evolution and constitution in the 14th edition of the *Encyclopaedia Britannica*. He already had modified some of his earlier views to deal with the special cases of white dwarfs and Trumpler stars and later he sketched the problems of differing luminosities for stars of populations I and II. He also dwelt upon the changes in theories of stellar evolution and of the internal constitution of the stars which would follow when an answer became available to the question 'What keeps the stars shining?' He lived to see the answers given by Bethe and others to this question—nuclear processes in the atom and the conversion of mass into energy deep in the star's interior. Russell saw his theory of stellar evolution give way to new and revolutionary views—the common fate of all such theories, as fresh knowledge comes along. Throughout he had shown that he retained a flexible outlook and he was quick to seize upon new points as they arose.

VI. Solar problems

Russell's first solar paper was a short note on cooling by expansion of rapidly rising gases in sunspots. His conclusion was that the gases must expand to more than thirty times their original volume at a base of the spot vortex where the temperature was above 20 000°C. He next became interested in ionization in the sun's atmosphere. He identified rubidium in the spectrum of sunspots, as predicted by Saha. He showed that the absorption of radiation in upper layers increased ionization by shifting an electron into a position from which it is more easily removed from the atom and also, in cases where lines of an ionized atom have been absorbed, by raising the ionized atom to a level where it is less likely to combine with an electron. He used this theory to explain certain anomalies in the spectrum.

Simultaneously with Milne he gave a correction to Saha's formula for the case of second stage ionization and he extended Saha's theory to a mixture of elements. He verified his theoretical results by a comparison of the spectra of sunspots and the reversing layer. He next, with J. Q. Stewart, studied the pressures to be found at the sun's surface. From the sharpness of the lines sensitive to pressure, the widths of the Fraunhofer lines, the ionization and chemical equilibria shown by the spectrum he showed that the pressure in the photosphere was less than 0·01 atm and not more than 0·0001 atm in the reversing layer. Lines produced in stellar atmospheres are substantially the same as those produced under vacuum conditions. Similar results were obtained by R. H. Fowler and Milne and published between the dates when Russell's paper was read and published. An increase of density by a factor 10^5 between the chromosphere and photosphere indicates a depth of 200 km for the reversing layer. The transition is rapid enough to account for the sharp edge of the sun and gradual enough for the darkening at the limb.

With Adams and C. E. Moore Russell carried out a calibration of Rowland's scale of intensities, R, using the internal intensities of 228 multiplets to give an empirical formula for the relative number of atoms N corresponding to lines of different intensities. The formula obtained was $\log N = B \log A$, where A is a function of R ranging from $-1\cdot 80$ for $R = -3$ to $4\cdot 0$ for $R = 40$: B a function of wavelength ranges from $1\cdot 41$ at 3000 Å to $0\cdot 82$ at 6900 Å. This work was extended by Russell and Adams to stellar spectra, using high-dispersion spectra from Mt Wilson observatory, a reconnaissance into new territory. This work led up to Russell's classical paper 'On the composition of the sun's atmosphere'. From his many spectroscopic researches (to be discussed below) Russell tabulated the energy of binding of atoms, neutral and ionized, in different quantum states, also he tabulated the ionization potentials of different elements and their most persistent lines. The presence and absence of elements in the spectrum were explained. The calibration of Rowland's scale and the quantitative work on line contours and equivalent widths started by Unsöld led to a study of the abundance of the elements and the relative proportions of the number of atoms of an element that are neutral and ionized. The relative abundance of 56 elements and 6 compounds was tabulated. The great abundance of hydrogen compared with helium, oxygen, the metallic vapours and free electrons was stated for the first time and its effect in explaining a number of astrophysical puzzles was pointed out. The 'Russell mixture' was widely adopted by other writers in their work on theoretical stellar models. The study of the composition of the sun's atmosphere was afterwards extended by Russell to the stars to determine the mean ionization in stellar atmospheres, ranging in temperature from 1700 to 25000°K and in electron pressure from 10^3 to 10^{-30} atm. Tables for hydrogen and metals are given for an assumed mixture of elements with temperature as one factor and electron pressure or gas pressure as the second factor concerned. A further paper on molecules in the atmospheres of the sun and stars, based on the solar abundance data, showed weakening of bands from spots to disk and from dwarfs to giants. The presence of metallic emission lines in long-period variables was attributed to the dissociation of compounds.

VII. Stellar atmospheres

As is seen above Russell's investigations on the atmosphere of the sun were extended to the stars. Further studies dealt with the problem of opacity in stellar atmospheres. This was due to individual action of neutral atoms, interaction of free electrons with neutral atoms and ions or to photoelectric ionization of atoms in various states of excitation. The intensity of lines in stellar spectra depends in part on the distances that we can see down in the atmosphere, that is on the laws of opacity. Russell gave equations relating the numbers of neutral and ionized atoms and the optical depth of an atmosphere consisting of two partially ionized elements and other neutral elements; or again for one element partially ionized the rest being neutral or wholly ionized; he examined the influence on the results of variation in relative abundances, of gravity and of temperature. In studying the changes along the temperature sequence he showed that the lines of any element could only pass through a maximum in the presence of some element more easily ionized. Russell also examined the effect of gravity as well as of pressure in the atmospheres of giants and dwarfs and accounted for a number of anomalies in the positions of the maxima of different elements in the temperature sequence. In his Halley Lecture at Oxford in 1933 he gave a full account of his theories on the composition of the stars and of the rules governing the changes of intensity of different elements along the main sequence and of atoms of the same element at different stages of ionization.

In a discussion of various stellar models Russell showed the effect of altering the opacity of the outer layers in fitting a stellar atmosphere on to a model. He also showed that for a star of given mass and radius there is only one value of Emden's polytropic index n. This index indicated the order of magnitude of the central condensation.

VIII. Laboratory spectra

In a number of the papers discussed in the last three sections Russell made use of his very wide knowledge of the spectra of the elements. Some account must now be given of the enormous amount of work that he did in analyzing the laboratory spectra of a large number of elements. His first major contribution to the subject in 1925 proved to be one of fundamental importance, a study with F. A. Saunders on 'New regularities in the spectra of the alkaline earths'. New series were found for calcium, barium and strontium, and new anomalous terms, where the inner quantum properties were normal while in some cases the combinatory properties were abnormal differing by one unit (or at times two) from the ordinary azimuth quantum number. The explanation put forward by the authors turned on the fact that some of the numerical values of these terms were negative, showing that a neutral atom might contain more energy than was required to ionize it. The explanation was that two valency electrons were displaced to outer orbits when a quantum of energy was received (or jumped together from outer to inner orbits when a quantum of energy was radiated) the atom acting as a whole. The paper contained a simple notation to include the ordinary multiplets and anomalous terms. This paper was Russell's most significant contribution to a subject which was to be greatly developed under his inspiration. In subsequent analyses of spectra the Russell-Saunders coupling between orbital momentum and electron magnetic spin proved of great value.

That Russell had been already studying carefully the whole subject of the analysis of spectra is clear from the fact that three more papers were published in the same year on quantitative rules governing the intensities of lines in multiplets (simultaneously with Sommerfeld and Hund), on the spectrum of ionized calcium (with Saunders) and on a 'List of ultimate and penultimate lines of astrophysical interest.' Russell in this laboratory work was ever linking his work closely with his astrophysical studies. There followed a spate of papers for the next twenty years and more, witnessing to the amazing energy of Russell and to his uncanny skill in picking out the essentials in a complex problem. As will be seen from the bibliography below many of these were joint papers; Russell's collaboration with others, though not always mentioned in the text of this biography, was very typical of his whole career. His most frequent co-worker was Dr Charlotte E. Moore (Sitterly), but over twenty well-known names figure as joint authors with him. In 1926 35 multiplets in the spark spectrum of iron were identified and a series of lines in the arc spectrum of magnesium. In 1927 the arc and spark spectra of scandium and yttrium were analyzed, the arc and spark spectra of titanium and the spectra of doubly ionized and trebly ionized titanium. Details agreed completely with Hund's theory, to which Russell makes constant reference in these papers; neutral scandium and ionized titanium closely satisfied Moseley's law and Zeeman effects were found to agree with Landé. Seventeen series were identified in neutral titanium, converging to seven limits, all terms in ionized titanium. In the same year Russell published a paper on groups of homologous lines in spectra of different elements which arise from similar electronic transitions in the atom; he took the group of elements from potassium to zinc to illustrate the steady shift shortward of the lines with an increase in atomic number. Series and approximate series limits were identified and the principal ionization potentials were given for each element in the group. Russell also gave an account of the method of the calculation of spectroscopic terms derived from equivalent electrons in which Pauli's exclusion principle was operative.

Russell maintained a high rate of output of papers during the two following years (1928-1929) when analyses were given of the spark spectrum of oxygen (confirming Bowen's identification of the forbidden nebular lines at 3727A), of neon and argon in the extreme ultra-violet, of the arc and spark spectrum of yttrium and of the arc spectrum of nickel. Russell also gave a report on the presence of 289 predicted lines of iron in the solar spectrum, lines not previously found in the laboratory. He further published with A. G. Shenstone and L. A. Turner a report on 'The notation for atomic spectra', a clarifying notation to meet a multiplet or level analysis, a configuration analysis and a series analysis. It was during these three busy years that he gave the Terry lectures on religion at Yale University, published the well-known standard text-book on *Astronomy* (with R. S. Dugan and J. Q. Stewart) and such papers as a new method for analyzing stellar spectra, the dynamical parallaxes of 1777 double stars, an article on stellar evolution in the *Encyclopaedia Britannica* and his classical paper on the composition of the sun's atmosphere.

In 1932 Russell returned to laboratory spectra with an analysis of lanthanum spectra (with W. F. Meggers) and with a discussion on perturbed series in line spectra for which he and A. G. Shenstone found a new series formula. Magnesium, radium and europium offered him further fields for study. In 1935 he gave in the George Darwin lecture to the Royal Astronomical Society a history of the analysis of spectra and the application to astronomy. He showed that Bohr's theory of line radiation in terms of transition between two energy states had held the ground for twenty years though the idea of definite orbits was being replaced by wave-mechanical considerations. He described Hund's theory as one of the most satisfactory in the whole realm of physics. He summarized the requisites for spectral analysis as the following: lists of wavelengths, intensities, temperature classification, self-reversal, diffuseness, Zeeman effect, isoelectronic sequences, and Hund's theory. The spirit in which he approached the subject was shown in his description of it as 'a glorified cross-word puzzle.'

Russell contributed to the Zeeman Jubilee volume an article (with H. D. Babcock) on the Zeeman effect in the arc spectrum of vanadium. He improved on his earlier work on the intensities of lines in multiplets, analyzed with various colleagues the first spectrum of vanadium, the third spectrum of cerium, the arc and spark spectra of europium and gadolinium, the arc spectrum of cobalt and in a long and important paper (with C. E. Moore) the arc spectrum of iron. He revised and brought up to date (in 1952) the data given in his paper on the constitution of the Sun's atmosphere and finally published in 1955 a paper on 'The first spectrum of barium'. He was chairman of the committee of the National Research Council on line spectra in the elements and there can be no possible doubt of his qualifications to hold that office, as an ardent and most competent worker himself and as one who inspired keenness and enthusiasm in others.

IX. MISCELLANEOUS PAPERS

There were very few branches of astronomy which Russell failed to illuminate with some valuable contribution. In addition to the eight groups of papers discussed above mention must be made of the following:

His lectures at the University of Virginia on 'The solar system and its origin' discussed the dynamical properties of the system and the difficulties that arise in the distribution of angular momentum for any tidal or collision theory for its origin. Russell favoured the idea that the sun might be the survivor of a binary system; its smaller companion is presumed to have collided with a passing star in the remote past, when material bodies were more closely crowded together. This pioneer work inspired a number of more recent workers in this field.

A contribution to the time-scale controversy of some thirty years ago is given in a paper on the superior limit to the age of the earth's crust, the problem being discussed in terms of radio-activity. He gave as his most reasonable guess for the age of the earth 4×10^9 years.

A paper on 'Some problems of sidereal astronomy', the first of a series of research surveys of the National Research Council well repays study to-day even though it was written as long ago as 1919. It deals with individual stars, galaxies, clusters and nebulae and in its clarity and completeness indicates how it was that Russell was such an inspiration to astronomers wherever he went. Russell was also a member of the small committee on celestial mechanics with E. W. Brown as chairman which reported to the National Research Council in 1922 on celestial mechanics.

With D. H. Menzel he discussed the terrestrial abundance of permanent gases. The conclusion reached was that most of the neon and nitrogen in the original atmosphere of the planet must have escaped in the first few years or even days of the planet's existence. The gases that have survived, e.g. oxygen, may have been a product of vegetation; carbon and water vapour would have entered into the composition of the molten magma.

Russell's interest in the early controversies aroused by Einstein's theory of relativity was shown in three papers which included a refutation of Majorana's theory of gravitation and a proof that neither radiation pressure between galaxies nor electrostatic repulsion could account for their relative motions.

One more paper must be mentioned if only because it reflects the fact that Russell was for a time in World War I a civil engineer carrying out researches under the Division of Science and Research of the Bureau of Aircraft Production. For his paper 'On the navigation of airplanes' Russell made observations at heights up to 16000 ft. and in aeroplanes flying at 105 miles per hour. He offered as always practical suggestions for working with different horizons and instruments and for swift reductions of observations.

In addition Russell's facile pen made him a frequent and welcome contributor to *Science* for many years and a regular monthly contributor for over forty years to the *Scientific American*, of which he remained a member of the Board of Editors up to 1943. His popularization of astronomy was maintained throughout at a very high level. He was very frequently called upon to give lectures for one foundation or another. Several have been mentioned in some detail above. One further set of lectures must be mentioned, 'Fate and freedom', the Terry lectures on 'Religion in the light of science and philosophy' delivered at Yale University. While accepting fully the mechanistic theory of nature, including human nature, Russell maintained that it left untouched all relations interpreted on the psychological level. He regarded the mechanistic but statistical hypothesis of our nature as not an enemy but an ally of morals and religion. The religion of reason and the religion of aspiration are very close together. He took as an analogy two realms in nature—matter and radiant energy and added a spiritual realm which might interact without disturbing the laws of conservation.

Other lifelong interests of Russell were ancient history and archaeology, botany and geology, climbing, boating and above all travelling. His position in the world of astronomy was recognized by many awards: the Gold Medal of the Royal Astronomical Society (1921), the Henry Draper Medal (1922), the Lalande Medal (1922), the Bruce Medal (1925), the Rumford Medal of the American Academy of Arts and Sciences (1925), the Franklin Medal (1934) and the Janssen Medal (1936). He was an honorary graduate of Dartmouth, Louvain, Harvard, Yale and an Honorary Fellow of King's College, Cambridge. He was a member of the National Academy of Sciences, and the American Academy of Arts and Sciences: he was President of the American Astronomical Society, the American Philosophical Society and the

American Association for the Advancement of Science. He was a Foreign Member of the Royal Society, Associate of the Royal Astronomical Society, Corresponding Member of the Académie des Sciences, Paris, Associate of the Académie des Sciences, Brussels, Socio Staniero of the Accademia Nazionale dei Lincei Rome. He served as President of the Commissions on Stellar Spectra and on the Constitution of the Stars of the International Astronomical Union and as a member of nine other commissions.

He married in 1908 Lucy Mary Cole of New York. She survived him with four children. He died on 18 February 1957 at Princeton after a long illness. It could be fairly stated of him that when his health began to fail a few years ago he had completed his life's work. But he retained an active interest in astronomical and spectroscopic problems right to the end.

One might quote in conclusion the tribute paid to him by his former student and colleague, Harlow Shapley:

> 'Dean of American astronomers, profound student of the physics of the stars and the structure of the atom, master in the interpreting to us of the whole domain of astronomical research, philosopher of cosmic evolution and man's place in the Universe.'

Acknowledgment is due to Professor Harlow Shapley, one of Russell's first graduate students, for help in biographical details and to Dr Charlotte E. Moore-Sitterly, for so many years Russell's close co-worker, for help in providing the following bibliography and in many other ways.

F. J. M. STRATTON

BIBLIOGRAPHY

1898. A new graphical method of determining the elements of a double-star orbit. *Astron. J.* **19**, 9 and *Popular Astronomy*, **6**, 149.
1898. Elements of the planet DQ. *Astron. J.* **19**, 147.
1899. Elements and ephemeris of Eros. *Astron. J.* **20**, 8, 31, 134.
1899. The atmosphere of Venus. *Astrophys. J.* **9**, 284.
1899. The densities of variable stars of the Algol type. *Astrophys. J.* **10**, 315.
1899. Measures of the diameter of Jupiter. *Astron. J.* **20**, 77.
1900. The great inequality of Eros and the Earth. *Astron. J.* **21**, 24.
1900. The general perturbations of the major axis of Eros, by the action of Mars. *Astron. J.* **21**, 25.
1902. An improved method of calculating the orbit of a spectroscopic binary. *Astrophys. J.* **15**, 252.
1902. Shadows cast by starlight. *Popular Astronomy*, **10**, 242.
1905. (With A. R. HINKS.) Determinations of stellar parallax from photographs made at the Cambridge Observatory. *Mon. Not. R. Astr. Soc.* **65**, 775.
1905. The parallax of Lalande 21185 and γ *Virginis* from photographs taken at the Cambridge Observatory. *Mon. Not. R. Astr. Soc.* **65**, 787.

1906. On the light variations of asteroids and satellites. *Astrophys. J.* **24**, 1.
1906. The parallax of 8 stars from photographs taken at the Cambridge Observatory by A. R. Hinks and the writer. *Mon. Not. R. Astr. Soc.* **67**, 132.
1907. Note on paper on the probable distance of Orion. *Popular Astronomy*, **15**, 444.
1907. (With ZACCHEUS DANIEL.) Venus a luminous ring. *Astrophys. J.* **26**, 69.
1908. On the illumination of the dark side of Saturn's rings. *Astrophys. J.* **27**, 230.
1909. Elements and ephemeris of Daniel's comet. *Astron. J.* **26**, 46.
1910. On the origin of binary stars. *Astrophys. J.* **31**, 185.
1910. Determinations of stellar parallax. *Astron. J.* **26**, 147.
1910. Mass ratios in the systems of Krueger 60 and Castor. *Astrophys. J.* **32**, 363.
1910. Some hints on the order of stellar evolution (Abstract) *Science*, **32**, 883.
1911. Position of the Moon determined photographically. *Harvard Coll. Obs. Ann.* **72**, 1.
1911. A study of visual binary stars. (Abstract.) *Science*, **34**, 523.
1912. On the determination of the orbital elements of eclipsing variable stars. *Astrophys. J.* **35**, 315 and **36**, 54.
1912. The eclipsing variables W *Centauri* and W *Ursae Majoris*. (Abstract.) *Popular Astronomy*, **20**, 200.
1912. Elements of the eclipsing variables W *Delphini*, W *Ursae Majoris* and W *Crucis*. *Astrophys. J.* **36**, 133.
1912. Comments on the parallaxes of the 61 *Cygni* group. *Astron. J.* **27**, 95.
1912. Stars probably belonging to the 61 *Cygni* group discovered by Mr Benjamin Boss. *Astron. J.* **27**, 96.
1912. (With HARLOW SHAPLEY.) On darkening at the limb in eclipsing variables. *Astrophys. J.* **36**, 385.
1913. On the graphical representation of eclipsing variables. (Abstract.) *Science*, **37**, 30.
1913. On the luminous efficiency and colour-index of a black body at different temperatures. (Abstract.) *Science*, **37**, 646.
1913. Notes on the real brightness of variable stars. (Abstract.) *Science*, **37**, 652.
1913. On the reduction of polar trail plates. *Mon. Not. R. Astr. Soc.* **73**, 735.
1913. 'Giant' and 'Dwarf' stars. *Observatory*, **36**, 324.
1914. The probable order of stellar evolution. *Observatory*, **37**, 165.
1914. Relations between the spectra and other characteristics of the stars. *Popular Astronomy*, **22**, 275, 331; and *Nature, Lond.* **93**, 227, 252, 281.
1914. Solar spectrum and the earth's crust. *Science*, **39**, 791.
1914. (With HARLOW SHAPLEY.) Elements of the eclipsing variables Z *Draconis* and RT *Persei*. *Astrophys. J.* **39**, 405.
1914. A short method for determining the orbit of a spectroscopic binary. *Astrophys. J.* **40**, 282.
1914. (With HARLOW SHAPLEY.) On the distribution of eclipsing variable stars in space. *Astrophys. J.* **40**, 417.
1915. (With MARY FOWLER.) Photographic determinations of the position of the moon. *Harvard Coll. Obs. Ann.* **76**, 127.
1916. The stellar magnitudes of the sun, moon and planets. *Astrophys. J.* **43**, 103.
1916. On the albedo of the planets and their satellites. *Astrophys. J.* **43**, 173; and *Proc. Nat. Acad. Sci. Wash.* **2**, 74.
1916. The photographic brightness of the full moon. *Astrophys. J.* **44**, 128.
1916. Note on parallax of star in *Ophiuchus*. *Harvard Coll. Obs. Bull.* No. 617.
1916. Percival Lowell and his work. *The Outlook*, **114**, 781.
1917. The minimum radiation visually perceptible. *Astrophys. J.* **45**, 60.
1917. Preliminary parallax of Barnard's star of large proper motion. *Astron. J.* **30**, 73.
1917. The orbit of ε *Equulei*. *Astron. J.* **30**, 123.
1917. Orbit of Krueger 60. *Astron. J.* **30**, 131.
1917. (With MARY FOWLER, MARTHA C. BORTON and ERNESTINE FULLER.) Photographic determinations of the position of the moon. *Harvard Coll. Obs. Ann.* **80**, 151.

1917. (With Mary Fowler and Martha C. Borton.) Comparison of visual and photographic observations of eclipsing variables. *Astrophys. J.* **45**, 306.

1917. Masses of the stars. *Popular Astronomy*, **25**, 666.

1919. Variable stars. *Science*, **49**, 127.

1919. Orbit of γ *Ursae Majoris*, 21306. (Abstract.) *Popular Astronomy*, **27**, 97.

1919. Edward Charles Pickering. *Science*, **49**, 151.

1919. On the navigation of airplanes. *Publ. Astr. Soc. Pacif.* **31**, 129.

1919. On the sources of stellar energy. *Publ. Astr. Soc. Pacif.* **31**, 205.

1919. Some problems of sidereal astronomy. *Proc. Nat. Acad. Sci. Wash.* **5**, 391; and *Popular Astronomy*, **28**, 212, 264.

1920. (With H. L. Cooke.) A proposed method for carrying triangulation across wide gaps. *Science*, **51**, 211.

1920. Modifying our ideas of nature: The Einstein theory of relativity. *Smithsonian Report* for 1921, 197.

1920. The determination of the position of the moon by photography. *Publ. Astr. Soc. Pacif.* **32**, 105.

1920. Note on Strömberg's method of determining mean absolute magnitudes. *Publ. Astr. Soc. Pacif.* **32**, 243.

1920. On the origin of periodic comets. *Astron. J.* **33**, 49.

1920. The probable diameters of the stars. *Publ. Astr. Soc. Pacif.* **32**, 307 and *J.R. Astr. Soc. Canada,* **15**, 83.

1920. Note on the Sobral eclipse photographs. *Mon. Not. R. Astr. Soc.* **81**, 154.

1920. Planetary families of comets. *Nature, Lond.* **105**, 467.

1920. Masses of stars. *Nature, Lond.* **105**, 500.

1921. Radiation pressure and celestial motions. *Astrophys. J.* **53**, 1.

1921. Stellar evolution. *J. Brit. Astron. Ass.* **31**, 178 and *Popular Astronomy*, **29**, 541.

1921. A superior limit to the age of the earth's crust. *Proc. Roy. Soc.* A, **99**, 84.

1921. An obstacle to the use of artificial horizons at sea. *Mon. Not. R. Astr. Soc.* **81**, 417.

1921. Rubidium in the Sun. *Publ. Astr. Soc. Pacif.* **33**, 202.

1921. Observations of the aurora at the Lowell Observatory, May 14, 1921. *Science*, **54**, 183.

1921. On the accuracy with which mean parallaxes can be determined from parallactic and peculiar motions. *Astrophys. J.* **54**, 140.

1921. Note on cooling by expansion in sun-spots. *Astrophys. J.* **54**, 293.

1921. The properties of matter as illustrated by the stars. *Publ. Astr. Soc. Pacif.* **33**, 275.

1921. On Majorana's theory of gravitation. *Astrophys. J.* **54**, 334.

1922. (With D. L. Webster.) Note on the masses of the stars. *Mon. Not. R. Astr. Soc.* **82**, 181.

1922. On the orbital eccentricity of binary stars of very long period. (Abstract.) *Popular Astronomy*, **30**, 167.

1922. The theory of ionization and the sun-spot spectrum, *Astrophys. J.* **55**, 119.

1922. Recent advances in stellar astronomy. *Rice Inst. Pamphlet*, **9**, 41.

1922. On the calculation of masses from spectroscopic parallaxes. *Astrophys. J.* **55**, 238.

1922. Notes on ionization in the solar atmosphere. *Astrophys. J.* **55**, 354.

1922. (With E. W. Brown, G. D. Birkhoff and A. O. Leuschner.) Celestial mechanics. *Bull. Nat. Res. Coun. Wash.* **4**, 1.

1922. Dark nebulae. *Proc. Nat. Acad. Sci. Wash.* **8**, 115; and *Nature, Lond.* **110**, 81.

1922. The new international symbols for the constellations. *Popular Astronomy*, **30**, 469.

1923. Ionization and pressure in the reversing layers of the stars. (Abstract.) *Popular Astronomy*, **31**, 22.

1923. Some results of a study of visual double stars. (Abstract.) *Popular Astronomy*, **31**, 250.

1923. (With W. S. Adams and A. H. Joy.) A comparison of spectroscopic and dynamical parallaxes. *Publ. Astr. Soc. Pacif.* **35**, 189.

1924. Singlet series in the spark spectrum of aluminium. *Nature, Lond.* **113**, 163.

1924. Ultimate and penultimate lines in spectra. (Abstract.) *Popular Astronomy*, **32**, 229.

1924. Series in the arc and spark spectra of titanium. (Abstract.) *Popular Astronomy*, **32**, 230.

1924. (With J. Q. Stewart.) Pressures at the Sun's surface. *Astrophys. J.* **59**, 197.
1924. A new form of the exclusion principle in optical spectra. *Science*, **59**, 512.
1924. (With E. W. Brown, L. J. Comrie and W. S. Eichelberger.) Report of the Committee on eclipses. *Popular Astronomy*, **32**, 450.
1924. Notes on the Sun-spot spectrum. (Abstract.) *Popular Astronomy*, **32**, 620.
1924. (With R. K. Root.) A planetary date for Chaucer's Troilus. *Publ. of Modern Language Association of America*, **39**, 48.
1924. The application of modern physics to astronomy. *J.R. Astr. Soc. Canada*, **18**, 126, 137, 201, 233.
1924. (With K. T. Compton.) A possible explanation of the behaviour of the hydrogen lines in giant stars. *Nature, Lond.* **114**, 86.
1925. The intensities of lines in multiplets. *Nature, Lond.* **115**, 835.
1925. Problem of stellar evolution. *Nature, Lond.* **116**, 209.
1925. (With F. A. Saunders.) New regularities in the spectra of the alkaline earths. *Astrophys. J.* **61**, 38.
1925. The new physics and the stars. *Yale Review*, **14**, 463.
1925. A list of ultimate and penultimate lines of astrophysical interest. *Astrophys. J.* **61**, 223.
1925. The intensities of lines in multiplets I—Theory. II—Observed data. *Proc. Nat. Acad. Sci. Wash.* **11**, 314.
1925. (With F. A. Saunders.) On the spectrum of ionized calcium. *Astrophys. J.* **62**, 1.
1925. Note on the relation between the mass, temperature and luminosity of a gaseous star. *Mon. Not. R. Astr. Soc.* **85**, 935.
1926. Dispersed systems in astronomy. Article in J. Alexander's *Colloid Chemistry*, p. 444.
1926. (With Charlotte E. Moore.) On the winged lines in the solar spectrum. *Astrophys. J.* **63**, 1.
1926. Note on ionization in giant and dwarf stars. *Harvard Coll. Obs. Circ.* 291.
1926. On the problem of stellar evolution. (Abstract.) *Popular Astronomy*, **34**, 244.
1926. An outdoor optical experiment. *Science*, **63**, 616.
1926. A new term in the arc spectrum of magnesium. *Publ. Astr. Soc. Pacif.* **38**, 236.
1926. The use of models in an astronomical museum. *Natural History*, **26**, 399.
1926. Multiplets in the arc spectrum of iron. *Astrophys. J.* **64**, 194.
1927. The relation of physics to astronomy. (A symposium on *Physics in its relations,* with five others.) *Vassar College.*
1927. On the calculation of the spectroscopic terms derived from equivalent electrons. *Phys. Rev.* **29**, 782.
1927. (With William F. Meggers.) An analysis of the arc and spark spectrum of scandium. *Sci. Pap. U.S. Bur. Stand.* **22**, 329; and *Phys. Rev.* **29**, 606 A.
1927. (With R. J. Lang.) On the spectrum of doubly and trebly ionized titanium. *Astrophys. J.* **66**, 13.
1927. On the relations between period, luminosity and spectrum among Cepheids. *Astrophys. J.* **66**, 122.
1927. Related lines in the spectra of the elements of the iron group. *Astrophys. J.* **66**, 187.
1927. Series and ionization potentials of the elements of the iron group. *Astrophys. J.* **66**, 233.
1927. The arc and spark spectra of titanium. *Astrophys. J.* **66**, 283, 347.
1928. On the spark spectrum of oxygen. *Phys. Rev.* **31**, 27.
1928. (With K. T. Compton and J. C. Boyce.) The spark spectra of neon and argon. (Abstract.) *Phys. Rev.* **31**, 709.
1928. (With K. T. Compton and J. C. Boyce.) The spark spectrum of neon. *Proc. Nat. Acad. Sci. Wash.* **14**, 280.
1928. On the determination of dynamical parallaxes. *Astron. J.* **38**, 89.
1928. (With A. G. Shenstone.) A quick method of reducing prismatic spectra. *J. Opt. Soc. Amer.* **16**, 298.
1928. (With W. S. Adams.) A new method of determining temperatures and pressures in stellar atmospheres. (Abstract.) *Popular Astronomy*, **36**, 294.

1928. (With W. S. Adams and C. E. Moore.) A calibration of Rowland's scale of intensities for solar lines. *Astrophys. J.* **68**, 1.

1928. (With W. S. Adams.) Preliminary results of a new method for the analysis of stellar spectra. *Astrophys. J.* **68**, 9.

1928. On the advance of periastron in eclipsing binaries. *Mon. Not. R. Astr. Soc.* **88**, 641.

1928. (With J. C. Boyce and K. T. Compton.) Extreme ultra-violet spectrum of argon, excited by controlled electron impact. *Phys. Rev.* **32**, 179.

1928. (With C. E. Moore.) The presence of predicted iron lines in the solar spectrum and the terms in the arc spectrum of iron. *Astrophys. J.* **68**, 151.

1929. Stellar evolution. (14th edition *Encyclopaedia Britannica*).

1929. On meteoric matter near the stars. *Astrophys. J.* **69**, 49.

1929. (With I. S. Bowen.) Is there argon in the corona? *Astrophys. J.* **69**, 196.

1929. (With William F. Meggers.) An analysis of the arc and spark spectrum of yttrium. *J. Res. U.S. Bur. Stand.* **2**, 733.

1929. Facing the Universe. *The Outlook*, **152**, 19.

1929. (With A. G. Shenstone and L. A. Turner.) Report on notation for atomic spectra. *Phys. Rev.* **33**, 900.

1929. On the composition of the sun's atmosphere. *Astrophys. J.* **70**, 11.

1929. The arc spectrum of nickel. *Phys. Rev.* **34**, 821.

1929. (With C. E. Moore.) Dynamical parallaxes of 1777 double stars. *Astron. J.* **39**, 165.

1929. Learning from the eclipse. *The Outlook*, **152**, 381.

1930. Life in time and space. (A chapter in E. V. Cowdry's *Human Biology and Racial Welfare*, p. 3.)

1930. A note on Zeeman patterns. *Phys. Rev.* **36**, 1590.

1930. (With R. S. Dugan.) Apsidal motion in γ *Cygni* and other stars. *Mon. Not. R. Astr. Soc.* **91**, 212.

1931. More about Pluto. *J. R. Astr. Soc. Canada*, **25**, 27.

1931. (With R. d'E. Atkinson.) The stellar graveyard. *Nature, Lond.* **127**, 661.

1931. Composite polytropic gas spheres. *Mon. Not. R. Astr. Soc.* **91**, 739.

1931. Meteor crater. *Mus. Notes, Flagstaff.* **4**, 1.

1931. Notes on the constitution of the stars. *Mon. Not. R. Astr. Soc.* **91**, 951.

1931. The composition of the Sun. *Smithsonian Report* for 1931, 199.

1932. (With A. G. Shenstone). Perturbed series in line spectra. *Phys. Rev.* **39**, 415.

1932. Mean ionization in stellar atmospheres. *Astrophys. J.* **75**, 337.

1932. The master key of science; revealing the universe through the spectroscope. *Smithsonian Report* for 1932, 199.

1932. Note on Milne's theory of stellar structure. *Mon. Not. R. Astr. Soc.* **92**, 831.

1932. (With W. F. Meggers.) An analysis of lanthanum spectra (La I, La II, La III). *J. Res. U.S. Bur. Stand.* **9**, 625.

1932. New planetary discoveries. *J. R. Astr. Soc. Canada*, **26**, 391.

1933. *The composition of the stars.* (Halley lecture), Oxford Univ. Press.

1933. The constitution of the stars. *Science*, **77**, 65; and *J. R. Astr. Soc. Canada*, **27**, 375.

1933. The attraction of spheres. *Science*, **77**, 412.

1933. (With C. E. Moore.) On the masses of binary stars. (Abstract.) *Science*, **77**, 455.

1933. (With C. E. Moore.) On the masses of giant stars. (Abstract.) *Publ. Amer. Ass. Advanc. Sci.* **7**, 184.

1933. A rapid method for visual binary orbits. *Mon. Not. R. Astr. Soc.* **93**, 599; and *Observatory*, **56**, 210.

1933. (With D. H. Menzel.) The terrestrial abundance of the permanent gases. *Proc. Nat. Acad. Sci. Wash.* **19**, 997.

1934. The atmospheres of the stars. *J. Franklin Inst.* **218**, 127.

1934. Opacity formulae and stellar line intensities. *Astrophys. J.* **78**, 239.

1934. Molecules in the sun and stars. *Astrophys. J.* **79**, 317.

1934. (With H. D. Babcock and C. E. Moore.) Series lines of magnesium in the solar spectrum. *Phys. Rev.* **46**, 826.
1934. The spectrum and ionization potential of radium. *Phys. Rev.* **46**, 989.
1934. (With A. S. King.) The arc spectrum of europium. *Phys. Rev.* **46**, 1023.
1934. (With J. C. Boyce and A. G. Shenstone.) *Rydberg interpolation table.* Princeton Univ. Press.
1934. Astronomy. *American Year Book* 1934, 652.
1935. The absorption bands in the spectra of the outer planets. *J. R. Astr. Soc. Canada,* **29**, 51.
1935. The atmospheres of the planets. *Science,* **81**, 1; and *Nature, Lond.* **135**, 219.
1935. (With C. H. Payne Gaposchkin and D. H. Menzel.) The classification of stellar spectra. *Astrophys. J.* **81**, 107.
1935. Recent progress in astronomy. *Encyclopaedia Britannica* '*The World Today*', **2**, 42.
1935. (With H. D. Babcock.) The Zeeman effect in the arc spectrum of vanadium. Jubilee Zeeman volume *Pieter Zeeman 1865-1935.*
1935. The analysis of spectra and its applications in astronomy. *Mon. Not. R. Astr. Soc.* **95**, 610.
1935. The newer telescopes. *J. R. Astr. Soc. Canada,* **29**, 231.
1935. Astronomy and astrophysics. *American Year Book* 1935, 616.
1936. (With T. Dunham Jr. and R. Wildt.) Planetary atmospheres. *Mon. Not. R. Astr. Soc.* **96**, 376.
1936. Note on the motion of masses of gas seen near *Nova Herculis. Publ. Astr. Soc. Pacif.* **48**, 29.
1936. Tables for intensities of lines in multiplets. *Astrophys. J.* **83**, 629.
1936. Orbit of the binary ref. cat. 17h 31. *Astron. J.* **45**, 95.
1936. (With W. F. Meggers.) Term analysis of the first spectrum of vanadium. *J. Res. U.S. Bur. Stand.* **17**, 125.
1936. Polytropic index and photospheric conditions. *Mon. Not. R. Astr. Soc.* **97**, 127.
1936. Astronomy. *American Year Book,* 1936, 621.
1937. Model stars. *Bull. Amer. Math. Soc.* **43**, 49.
1937. (With R. B. King and R. S. Lang.) The third spectrum of cerium. *Phys. Rev.* **52**, 456.
1937. How did it all begin? From Adamson's '*The solar system and its origin*'. *The Sky,* **1**, 12.
1938. Fitting atmospheres to stars. *Mon. Not. R. Astr. Soc.* **98**, 199.
1938. (With C. E. Moore.) A comparison of spectroscopic and trigonometric parallaxes. *Astrophys. J.* **87**, 389.
1938. Emission lines in astronomical spectra. *Trans. International Astronomical Union,* **6**, 427.
1939. The place of approximate methods in astronomy. *Publ. Amer. Ass. Advanc. Sci.* **9**, 108.
1939. A natural scientist evaluates intellectual freedom. *Princeton Alumni Weekly,* **39**, 479.
1939. (With A. S. King.) The arc spectrum of europium. *Astrophys. J.* **90**, 155.
1939. Notes on ellipticity in eclipsing binaries. *Astrophys. J.* **90**, 641.
1939. Stellar energy. *Proc. Amer. Phil. Soc.* **81**, 295; and *J.R. Astr. Soc. Canada,* **33**, 287.
1940. The time-scale of the universe. *Science,* **92**, 19.
1940. (With R. B. King and C. E. Moore.) The arc spectrum of cobalt. *Phys. Rev.* **58**, 407.
1940. (With C. E. Moore.) The systematic and accidental errors of spectroscopic parallaxes. *Astrophys. J.* **92**, 354.
1940. (With C. E. Moore.) Neutral barium in the Sun. (Abstract.). *Publ. Amer. Ass. Advanc. Sci.* **10**, 64.
1941. The distribution of density within the stars. *Ann. N.Y. Acad. Sci.* **41**, 15.
1941. The cosmical abundance of the elements. *Science,* **94**, 375.
1941. (With W. Albertson and D. N. Davis.) The spark spectrum of europium. *Phys. Rev.* **60**, 641.
1942. Notes on eclipsing variables. *Astrophys. J.* **95**, 345.
1942. Spectral structure and ionization potential of gadolinium I. *Astrophys. J.* **96**, 11.
1942. The present state of the theory of stellar evolution. *Sci. Mon.* **55**, 233.
1943. Paramount and absolute obligations. *Religion in Life,* Summer issue.

1943. Determinism and responsibility. *Science*, **97**, 249.
1943. Physical characteristics of stellar companions of small mass. *Publ. Astr. Soc. Pacif.* **55**, 79.
1943. The orbit of 70 *Ophiuchi. Publ. Astr. Soc. Pacif.* **55**, 164.
1944. The borders of the humanities. *The Sewanee Rev.* **52**, 203.
1944. Notes on white dwarfs and small companions. *Astron. J.* **50**, 13.
1944. Provisional elements of the eclipsing binary V 444. *Cygni. Astrophys. J.* **100**, 213.
1944. (With C. E. Moore.) The arc spectrum of iron. *Trans. Amer. Phil. Soc.* **34**, 113.
1945. Intermediary elements for eclipsing binaries. *Astrophys. J.* **102**, 1.
1946. The determination of the elements of binary stars. *Popular Astronomy*, **54**, 162.
1946. Rectification of the light-curves of eclipsing variables. *Astrophys. J.* **104**, 153.
1947. The ivory tower and the ivory gate. *Physical science and human values.* Princeton Univ. Press.
1947. America's role in the development of astronomy. *Proc. Amer. Phil. Soc.* **91**, 10.
1947. Present needs in the study of eclipsing binaries. *Harvard Coll. Obs. Circ.* **451**.
1948. On the distribution of absolute magnitude in populations I and II. *Publ. Astr. Soc. Pacif.* **60**, 202.
1948. Preliminary solutions for eclipsing binaries from accurate observations. *Astrophys. J.* **108**, 53.
1948. Configurations of the low terms of Ti II. *Phys. Rev.* **74**, 689.
1948. The royal road of eclipses. *Harvard Coll. Obs. Monograph*, **7**, 181.
1948. Localized models and rectified light-curves for eclipsing variables. *Astrophys. J.* **108**, 388.
1948. The penumbral part of the reflexion effect in eclipsing variables—a reconnaissance. *Harvard Coll. Obs. Circ.* 452.
1950. The arc and spark spectra of gadolinium. *J. Opt. Soc. Amer.* **40**, 550.
1950. Estimation of ionization potentials by comparison with neighbouring elements. *J. Opt. Soc. Amer.* **40**, 618.
1952. Introductory remarks to the Symposium on the Hertzsprung-Russell diagram, *Astron. J.* **57**, 1.
1952. Spectra in the laboratory and the stars, *Helwan Publ.* 1952.
1952. (With J. E. Merrill.) The determination of the elements of eclipsing binaries. *Princeton Univ. Contrib.* No. 26.
1952. (With C. E. Moore.) Binding energies for electrons of different types. *J. Res. U.S. Bur. Stand.* **48**, 61.
1954. Magellanic clouds XIII. Comparison of Magellanic and galactic eclipsing variables. *Proc. Nat. Acad. Sci. Wash.* **40**, 549.
1955. (With C. E. Moore.) The first spectrum of barium. *J. Res. U.S. Bur. Stand.* **55**, 299.
1957. Eclipsing variables in the Magellanic clouds. *Vistas in Astronomy* II, 1177.
Monthly articles in *Scientific American* from March 31, 1900 to August 1943.

Books

1911. *Determinations of stellar parallax.* Carnegie Inst. Washington, Publ. No. 147.
1927. *Fate and freedom.* Yale University Press.
1927. (With R. S. Dugan and J. Q. Stewart.) *Astronomy.* Ginn and Co., Boston (1938. Supplement to Vol. II.) (1945. Vol. I revised.)
1935. *The solar system and its origin.* New York: Macmillan Co.
1940. (With C. E. Moore.) *The masses of the stars with a general catalogue of dynamical parallaxes.* Astroph. J. Monograph.

Smith

WILLIAM WRIGHT SMITH

1875-1956

Sir William Wright Smith, the eminent botanist, who was President of the Royal Society of Edinburgh from 1944 to 1949, died on 15 December 1956, in his eighty-second year.

For thirty-four years he held the dual appointment of Regius Professor of Botany in the University of Edinburgh and Regius Keeper of the Royal Botanic Garden, Edinburgh; he was also Queen's Botanist in Scotland.

Born at Parkend near Lochmaben on 2 February 1875, the son of a Dumfries-shire farmer, he early acquired the interest in living things and a love for the country, which (though he was to spend the greater part of his life in Edinburgh) remained predominantly with him all his days.

His school was the Dumfries Academy where he went till the age of sixteen, when he left for Edinburgh as first University Bursar. Every day he had to travel to school by train, yet he found time to explore his native countryside, and his regard for natural history was by no means confined to plants. For example, he enjoyed watching birds and fishing, or, with one or two companions, guddling for trout or, again, in a leisure hour lying on some sunny bank by a convenient rabbit warren with book and gun. Though not robust he played conventional games, and he was fond of cycling, sometimes covering long distances, once at least more than a hundred miles in one day.

Later, when living in Edinburgh, there was nothing that gave him more pleasure than to escape for a day from the atmosphere of the town, especially if it were to walk the hills with dog and gun. Then, should he chance to meet with some lone shepherd, he would delight to question him on the welfare of his flock and to discuss authoritatively with him the problems of lambing, while matching his words to the native brogue. By that time (though he remained active enough to play tennis until he was over seventy) he had long given up cycling, and he himself never drove a car, for he had no mechanical aptitude and a marked aversion from mechanical contrivances of every sort.

When he left school in 1891, it was his intention to become a school teacher. With this end in view, while studying for an Arts degree at the University of Edinburgh, he attended the Diploma Course for Teachers at the Moray House Training College. His studies were not restricted to the then conventional subjects; in addition to English and classics he devoted attention to botany and geology and also took classes in chemistry and zoology as subsidiary subjects. When he had graduated in 1896, he became a schoolmaster, as he had intended to do, and served with the Edinburgh School Board for a period of six years.

Thereafter, his interest in the natural sciences led to his appointment, in 1902, as assistant to Professor Sir Isaac Bayley Balfour and Lecturer in the Department of Botany of the University of Edinburgh.

The Department, then a small one, did not permit of a member of the staff concentrating upon the one aspect of his subject that might most appeal to him. Smith's duties as a lecturer were varied; for a time his principal subject was cryptogamic botany and—what appealed to him more—he was also responsible for the conduct of excursions in order to teach students the characteristic features of different types of vegetation and to recognize plants in the field. Often he would set out with a band of students for the summit of Ben Lawers or some other mountain top, thus, in the course of time, becoming familiar with the botany of the greater part of Scotland.

In 1907 the opportunity came to him to go to India and he left Edinburgh to take up an appointment under the Government of Bengal—as Curator of the Herbarium in the Royal Botanic Garden, Calcutta. He served for four years in India and twice during that period was called upon to act as Superintendent of the Garden and as Director of the Botanical Survey of India.

This experience was a valuable one for not only did he gain a wide knowledge of the floras of India and Burma, but he was able also, in the diversified flora of the Calcutta Garden, to study tropical vegetation from all parts of the world; moreover the Lloyd Botanic Garden, Darjeeling was also in his care.

Part of the management of every scientific garden is bound up with the investigation of economic problems connected with plants, but in Calcutta, the Superintendent had the unusual responsibility of two large Government Cinchona Plantations in the Darjeeling District, with the Government factory at Mungpu for the extraction of the alkaloid quinine. The distribution and sale of the commercial products were also supervised by him. He consequently acquired a wide administrative experience.

As Director of the Botanical Survey of India, his outstanding work pertained to the exploration of several of the remoter valleys of the Sikkim Himalaya, which botanists had never visited, although the flora of the eastern Himalayas was by then comparatively well known.

Firstly, in 1909, he travelled to the upper reaches of the Llonakh and Zemu rivers, north of the Kinchenjunga glaciers, an area in the north-west of Sikkim adjoining Nepal and Tibet, which Hooker had failed to reach because of an impenetrable growth of *Rhododendron* forest. This inclement and inhospitable region, difficult of access, with no roads, no bridges and no houses, only the yak-hair tents of nomad Tibetan herdsmen, and no inhabitants for nine months of the year, he explored most thoroughly. Secondly, the following year saw him in south-east Sikkim and in the Chumbi Valley, Tibet, where he continued similar reconnaissances. Both these expeditions afforded a wealth of botanical material for systematic study upon which he based the detailed accounts of the botany of this region, published in two voluminous numbers of the *Records of the Botanical Survey of India*.

He returned to Edinburgh in 1911 on the invitation of Sir Isaac Bayley

Balfour to become deputy keeper of the Edinburgh Garden. In India he had acquired the broad outlook that was reflected in all his later work. Just as he arrived home, the products of George Forrest's second expedition to Western China had reached Edinburgh and the task of sorting out this mass of material was one which Smith was able to undertake with unusual facility because of his Indian experience. The critical examination of botanical specimens with many new species to be described was for him merely to continue and expand the work he had already begun. Furthermore, as to the raising from seed of plants new to cultivation, having already encountered many allied species in their native habitats, he had an intimate knowledge of the treatment they would require.

For the next twenty years Forrest continued to collect on the borders of Burma and Tibet and in Yunnan, until he had completed seven expeditions and died in Tengueh in 1931, and thus Smith was provided with material for many years and much of his subsequent research was concerned with the plants of those regions.

Meanwhile, in 1922, he had been appointed to succeed Sir Isaac Bayley Balfour as Regius Professor of Botany in the University of Edinburgh and as Regius Keeper of the Royal Botanic Garden, Edinburgh, an office which he held until his death brought an end to a long and close association with the Garden, extending over a period of more than fifty years.

In his long service he seldom took leave: the one long break from regular duty in the Garden occurred during the first World War, when he was seconded to the Timber Supply Department as officer in charge of labour for Scotland. Subsequently, desirous of promoting the interests of forestry and agriculture in Scotland, he served for a time on the Scottish Consultative Committee for Forestry and also on the Agricultural Research Council.

As a botanist, Smith was essentially a systematist of the older school. His chief interest lay in the naming and classification of plants and he was well served by the unprecedented wealth of new material which was arriving from Western China during the first half of the present century and by which our garden flora was so greatly enriched. It was a happy chance that brought into close co-operation the two distinguished botanists, Balfour and Smith, with the enthusiastic collectors, Forrest, Farrer and Ward, who rivalled each other in their search for new plants. In the earlier days of the partnership, the genera *Primula* and *Rhododendron* were the perquisites of the Professor, the rest fell to his assistant Smith, who described no less than some 550 new species and several new genera. His 'Diagnoses specierum novarum chinensium in herbario Horti Regii Botanici Edinburgensis Cognitarum' fill the greater part of several volumes in the *Notes from the Royal Botanic Garden, Edinburgh*, and he covered a remarkable range, as a glance at the full bibliography of his works will show. His main contributions to the problems of classification in general will be found in the Master's Lectures read to the Royal Horticultural Society in 1935, which were entitled 'Problems in the classification of plants' and published in two parts in the *Journal* of the Society for that year; also in the Hooker Lecture read to the Linnean Society of London and published in their *Journal* in 1933, under the title 'Some aspects of the bearing of cytology on taxonomy'.

On succeeding to the Keepership in 1922 he carried on the work that Balfour had begun, and though his name does not appear on the title page, it was he who was largely responsible for the publication of *The species of Rhododendron* in 1930. Thereafter, however, his main interest lay in the genus *Primula*. He had described many new species and in later years began to monograph the genus. With the assistance of Dr H. R. Fletcher, he completed this work, section by section, in a series of papers published in the *Transactions of the Royal Society of Edinburgh* and in the *Transactions of the Botanical Society of Edinburgh*.

As Professor of Botany, he was, in the first place, a notable teacher. He enjoyed most of all perhaps lecturing to the large medical class of 200 students which met, for many years at the early hour of 8 a.m., in the summer term. He made his subject a living one, illustrative of the processes of life, and linking the study of plants to its practical application in medicine. His lectures were always copiously illustrated by a series of diagrams hung upon screens behind him and by a large selection of living plants brought in from the garden and spread out in front of him.

In the practical classes it was his custom to move about the benches satisfying himself that students, in learning to distinguish and recognize the characteristic features of the objects set before them, were also beginning fully to understand the importance of precise and accurate observation. At the same time it gave him the opportunity of making personal contact with each of them individually, and he had a remarkable memory for names and faces. When a former student returned to visit him, even after a lapse of twenty years or more, he would often at once recall, not merely his name, but also odd details of his antecedents and history.

With advanced students his first aim was to ensure that they should have a comprehensive knowledge of the plant kingdom as a whole before turning to the special study of some particular branch of their subject. He himself taught systematic botany to the senior classes, making the fullest use of the ample material that the Botanic Garden, with its plant houses and herbarium, could afford.

A special course on forest trees for forestry students, planned as an introduction to the varied vegetation of a tropical forest, was a boon to those who went abroad and many, when home on leave, spoke with appreciation of the training they had received.

It was his custom, on taking leave of a class at the final meeting of the session, to invite students to keep in touch with him, commending those who might be going abroad to interest themselves in the local flora and to send seed to the garden of rare or interesting plants they might come across. Consequently he maintained a wide correspondence and from time to time, unexpected packages of seed of unusual plants would reach the Garden, sent from all parts of the world.

As Regius Keeper, with the scientific aspect in the forefront, but with an eye to æsthetic arrangement, he did much to enhance the beauty, lay-out and reputation of the Garden.

Commonly in a botanic garden plants are grouped according to their relationships or by their countries of origin. Formal arrangement was reduced to a minimum at Edinburgh. A small section for students displayed representative plants in their natural orders, but for the rest the guiding principle was that a plant should be put in the position where it was most likely to flourish.

Reference has already been made to Smith's taxonomic work, much of which was concerned with plants of high horticultural merit. Not only did he name many such new plants, but he also played a leading part in their establishment in cultivation.

The number of seedlings raised by subscribers to the later expeditions was enormous, far byond the capacity of the normal garden and room had to be found for them in the policies beyond the garden walls. Thus the modern woodland garden came into being and the Woodland Garden at Edinburgh was one of the earliest, is one of the most æsthetically pleasing and one of the richest of its type.

With his unique knowledge of the outstanding genera—*Primula*, *Rhododendron*, *Meconopsis*, *Lilium*, *Gentiana*—and of the Sino-Himalayan flora, Smith was responsible for much of this new development.

Visitors from all over the world came to see the garden and to seek his advice, which he always gave readily, and it was a pleasure to him to show visitors round, whether a royal personage or working gardener, provided some sign of intelligent interest were shown. He assumed, as if instinctively, an equality with all sorts of men, whether colleague in the University, high-ranking government official, junior assistant or unskilled labourer. In the day-to-day business with workmen in the Garden accustomed to the Scottish vernacular he would use their own vocabulary and, talking to them in their own language, gain their regard and affection and a strong hold over them.

With Lady Smith he was invited to visit America in 1939 to open the Twenty-sixth International Flower Show in New York and to judge at Flower Shows in Boston and Philadelphia, as well as to attend a round of banquets given in their honour.

At the end of the tour, he was asked by a lady reporter the inevitable question—which of the three shows he thought the best, to which he replied —'You have heard what Oliver Wendell Holmes said about the bear pit. Well, my dear, I'm not putting my foot in it', a reply which earned for him the rightful reputation that he had 'showed himself to possess good Scotch wit and no small measure of Scotch canniness.'

He received many honours. His knighthood in 1932 and his election as a Fellow of the Royal Society in 1945. He was given the honorary degree of D. és Sc. by Toulouse University and an LL.D. by the University of Aberdeen. The Royal Society of Edinburgh awarded him the Makdougall-Brisbane prize; The Royal Horticultural Society the Victoria Medal of Honour in

1925, and the Veitch Memorial Medal in 1930; he succeeded Dr A. B. Rendle as the Society's Professor of Botany in 1938 and was elected a Vice-President in 1944. He was an Honorary member of the American Academy of Arts and Sciences and in 1952 the Massachusett's Horticultural Society awarded him the George Robert White Medal of Honour.

He was a man of great tact, generous by nature, loved and esteemed by a large number of friends and acquaintances, a number undiminished though he outlived many of his contemporaries, for he had the rare faculty of enlisting new friendships while still maintaining the old.

It was typical that, although he was in India for only four years, he remembered his Indian staff or their sons or grandsons and, from time to time, would write to them or be visited by some of them.

It was typical also that not long ago he should have re-read the whole of the novels by Sir Walter Scott, or to find him with a volume of Tacitus or a manual on chess, or that recently he should be reading Spanish although unfamiliar either with the country or the people.

His personal charm and ease of manner fortified him with many friends. His wide reading, his retentive memory, his aptitude as a speaker on formal occasions, and, for other occasions when he could relax, his rich and ready fund of stories, always apt and often Scottish and told in the broad Scots tongue, are things to be remembered.

He leaves a widow and three daughters.

J. M. Cowan

BIBLIOGRAPHY

The selected bibliography has been compiled to show the publications collected under the titles of the journals in which they appeared.

Records of the Botanical Survey of India

1911. (With G. H. Cave.) The vegetation of the Zemu and Llonakh valleys of Sikkim, **4**, No. 5, 141.

1911. Some additions to the flora of the Eastern Himalaya, **4**, no. 5, 261.

1911. Some additions to the flora of Burma, **4**, No. 5, 273.

1911. Four new species of the Compositae from Southern India and a new *Justicia* from Assam, **4**, No. 5, 323.

1913. The Alpine and Sub-Alpine vegetation of South-East Sikkim, **4**, No. 7, 323.

1913. New Indian *Didymocarpi*, **6**, No. 2, 41.

1913. (With S. C. Banerji and M. S. Ramaswami.) Two decads of New Indo-Burmese species, **6**, No. 2, 29.

1913. (With G. H. Cave.) A note on the Himalayan species of *Daphne*, **6**, No. 2, 45 (with plate).

1914. (With G. H. Cave.) Note on the East Himalayan species of *Alangium*, **6**, No. 4, 93 (with plates).

1914. Species Novae Plantarum in Herbario Horti Reg. Calcutt. Cognitarum, **6**, No. 4, 99.

Journal and Proceedings, Asiatic Society of Bengal

1911. Plantarum Novarum in Herbario Horti Regii Calcuttensis Cognitarum Decas, **7**, No. 3, 69.
1911. Note on *Sterculia alata* Roxb. var. *irregularis*—a remarkable instance of leaf variation, **7**, No. 3, 85 (with plate).
1911. A new gentian and two new swertias from the Eastern Himalaya, **7**, No. 3, 77 (with 2 plates).

Notes from the Royal Botanic Garden, Edinburgh

1912. New Burmo-Chinese species of *Didymocarpus*, **5**, 149 (with 5 plates).
1912. New species of *Craibiodendron*, **5**, 157 (with 3 plates).
1920. Notes on certain Asiatic Styracaceae, **12**, 231.
1920. Species of *Styrax* in Herb. Léveillé, **12**, 237.
1921. A new *Chelonistele* from Mt. Kinabalu, Borneo, **13**, 188.
1921. New orchids from Yunnan and Northern Burma, **13**, 189.
1922. Two new West African ceropegias, **13**, 307.
1926. A new *Omphalogramma* from Yunnan, **15**, 97.
1931. The contribution of China to European gardens, **16**, 215.
1931. Additions to the genus *Primula*, **16**, 223.
1933. A new *Primula* from S.W. China, **18**, 63.
1934. Two new primulas, **18**, 181 (with plate).
1935. What is *Primula Wallichiana* O. Ktze.? **18**, 274.
1936. New primulas from the Himalaya, Tibet, China and Burma, **19**, 167.
1937. New Primulaceae from Southern Tibet, **19**, 199.
1938. Additions to the *Primula*-flora of Tibet, **19**, 309.
1913-21. Diagnoses Specierum novarum chinensium in herbario Horti Regii Botanici Edinburgensis cognitarum. I-L, **8**, 105, 1913; LI-CII, **8**, 173, 1914; CIII-CL, **8**, 313, 1915; CLI-CCL, **9**, 71, 1916; Species Asiaticae, CCLI-CCCL, **10**, 1, 1917; CCCLI-CCCC, **10**, 167, 1918; CCCCI-CCCCL, **11**, 191, 1919; CCCCLI-D, **12**, 191, 1920; DI-DL, **13**, 149, 1921.
1915. (With I. B. Balfour.) *Moultonia*, A new genus of Gesneraceae from Borneo, **8**, 349 (with 2 plates).
1915. (With I. B. Balfour.) *Beesia*, A new genus of Ranunculaceae from Burma and Yunnan, **9**, 63 (with plate.)
1923. (With G. Forrest.) New Primulaceae, **14**, 31.
1927. (With G. Forrest.) Some new Asiatic Primulaceae, **15**, 247 (with plate).
1928. (With G. Forrest.) The sections of the genus *Primula*, **16**, 1 (with 2 plates): Reprint with alterations in *J. Roy. Hort. Soc.* **54**, 2.
1914. (With J. H. Lace.) Three Indo-Burmese rhododendrons, **8**, 213 (with 3 plates).
1926. (With F. K. Ward.) New species of *Primula* from Tibet, **15**, 69.
1927. (With F. K. Ward.) Additional species of *Primula* from the Burma-Tibet Frontier, **15**, 299.

Transactions of the Botanical Society of Edinburgh

1913. (With W. G. Craib.) A new *Pleurospermum*, **26**, part 2, 154.
1921. (With W. E. Evans.) *Craigia*, a new genus of Sterculiaceae, **28**, part 2, 69 (with plate).
1916. (With G. Forrest.) New garden dracocephalums from China, **27**, part 1, 89.

1917. (With J. Small.) *Cavea.* A new genus of the Compositae from the East Himalaya, **27**, 2, 119 (with plate).
1922. (With J. Small.) *Formania.* A new genus of the Compositae from Yunnan, **28**, part 3, 91 (with plate).
1922. (With J. Small.) *Parasenecio.* A new genus of the Compositae from China, **28**, part 3, 93 (with plate).
1941. (With H. R. Fletcher.) New species of Alpine primulas, **33**, part 2, 107.
1941. (With H. R. Fletcher.) The genus *Primula.* Section Candelabra Balf. f. **33**, part 2, 122.
1942. (With H. R. Fletcher.) The genus *Primula.* Section Amethystina, **33**, part 3, 209.
1942. (With H. R. Fletcher.) The genus *Primula.* Section Minutissimae, **33**, part 3, 227.

Kew Bulletin

1911. Decades Kewenses, **62**, No. 8, 344, 347.
1912. Decades Kewenses, **65**, No. 4, 198.

Transactions of the Royal Society of Edinburgh

1942 (With H. R. Fletcher.) The genus *Primula.* Section Nivales, **60**, part 2, 563, No. I.
1943. (With H. R. Fletcher.) The genus *Primula.* Section Farinosae, **61**, part 1, 1, No. XVII.
1944. (With H. R. Fletcher.) The genus *Primula.* Section Petiolares, **61**, part 2, 271, No. X.

Transactions of the Botanical Society of Edinburgh

1905. Note on *Arenaria tenuifolia* Linn. as a Scottish plant, **23**, part 1, 113.
1909. Notes on the flora of the Scilly Isles, **24**, part 1, 36.
1911. A new *Leycesteria*, **24**, part 3, 173 (with plate).
1911. *Borthwickia*, A new genus of Capparidaceae, **24**, part 3, 175 (with plate).
1913. Two new Himalayan Primulas from the Chumbi Valley, **26**, part 2, 118.
1913. A new and peculiar *Astragalus* from the Tibetan Frontier, **26**, part 2, 178.
1914. Note on *Rhododendron cyanocarpum* Franch, **26**, part 3, 274.
1914. An Himalayan variety of *Plumbagella micrantha* Spach., **26**, part 3, 277.
1914. A tuberous *Senecio* from China, **26**, part 3, 279.
1916. Note on *Parasyringa*, A new genus of Oleaceae, **27**, part 1, 93.
1919. *Whytockia*, A new genus of Gesneraceae, **27**, part 4, 338 (with plate).
1922. Notes on Chinese lilies, **28**, part 3, 122 (with 4 plates).
1924. On *Primula pauliana*, **29**, part 1, 8 (with plate).

The Journal of the Linnean Society of London

1942. (With H. R. Fletcher.) The section Soldanelloideae of the genus *Primula*, **52**, No. 344, 321.
1942. (With H. R. Fletcher.) New Asiatic primulas, **52**, No. 344, 336.

Miscellaneous papers

1933. The Hooker Lecture: Some aspects of the bearing of cytology on taxonomy. *Proc. Linn. Soc., Lond.* **145**, part 4.
1934. The Royal Botanic Garden and its value to the community. *Association of Superintendents of Parks and Botanic Gardens. Report 7th Annual Conference.*

1936. Masters Lectures 1935: Problems in the classification of plants I. *J. Roy. Hort. Soc.* **61**, part 2, 77.
1936. Problems in the classification of plants II. *J. Roy. Hort. Soc.* **61**, part 3, 117.

Curtis's Botanical Magazine

1935. *Primula szechuanica*, **158**, 9384.
1935. *Primula Wollastonii*, **158**, 9401.
1935. *Primula amethystina*, subs. *brevifolia*, **158**, 9410.

Proceedings of the Linnean Society of London

1940. *Primula obtusifolia* Royle. *Proc. Linn. Soc., Lond.* **153**, part 1.
1941. On *Primula erratica* W.W. Sm. *Proc. Linn. Soc., Lond.* **153**, part 1.

Obituary Notices

1923. Sir Isaac Bayley Balfour. *Trans. Bot. Soc. Edinb.* **28**, part IV, 192 (with portrait).
1932. George Forrest, 1873-1932. *Rhod. Soc. Notes*, **3**, 271; Repr. in *R.H.S. Journal*, **57**, part 2.
1938. Sir Herbert Eustace Maxwell, 1845-1937. *Obit. Not. Roy. Soc.* **2**, 387.
1938. Leonard Cockayne. *J. N.Z. Inst. Hort.* **8**, no. 1, 1938.
1939. Henry Halcro Johnston. *Proc. Roy. Soc. Edinb.* **59**, 270.
1941. John Rutherford Hill. *Trans. Bot. Soc. Edinb.* **33**, part 2, 182.

The Primula monograph was published as follows:

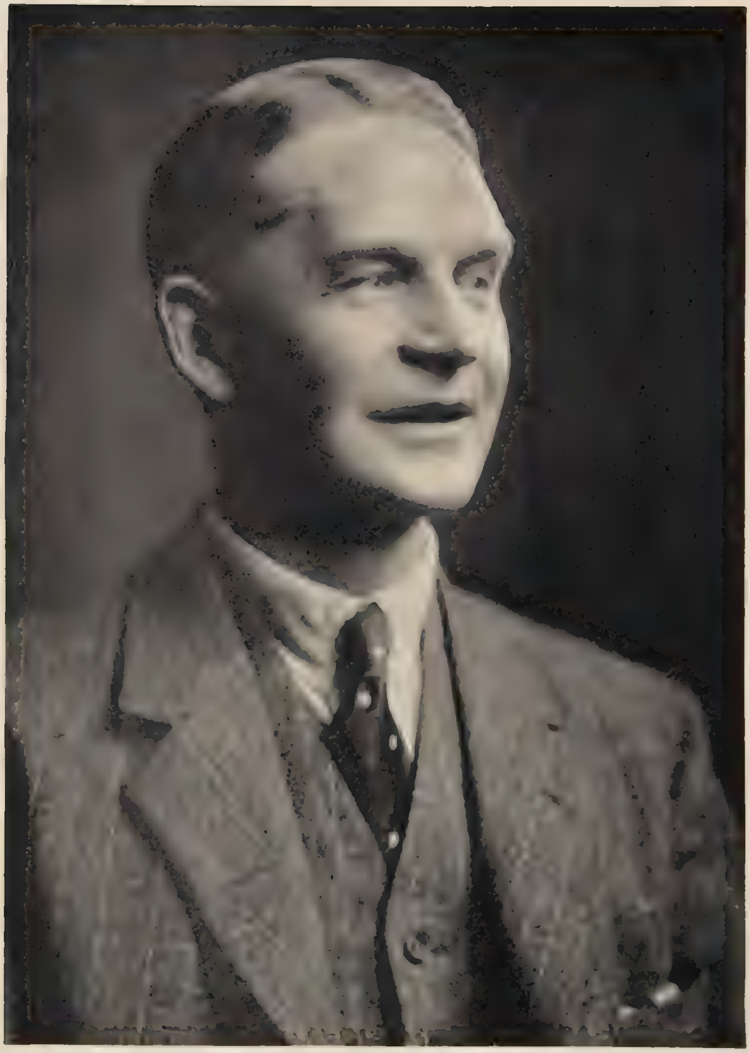
'The sections of the genus *Primula*'. By W. W. Smith and George Forrest in *Notes from the Royal Botanic Garden*, **16** (1928), 1-50: *J. Roy Hort. Soc.* **54** (1929), 4-49 (Key to sections only).

All the following sections published with H. R. FLETCHER:

1942. *Amethystina* section. *Trans. Bot. Soc.* **33**, 209-226.
1948. *Auricula* section. *Trans. Roy. Soc., Edinb.* **61**, 650-686.
1942. *Bella* section. *Trans. Bot. Soc.* **33**, 227-266.
1946. *Bullatae* section. *Trans. Roy. Soc. Edinb.* **61**, 454-463.
1941. *Candelabra* section. *Trans. Bot. Soc.* **33**, 122-181.
1944. *Capitatae* section. *Trans. Bot. Soc.* **34**, 149-158.
1946. *Carolinella* section. *Trans. Roy. Soc. Edinb.* **61**, 463-467.
1944. *Cortusoides* section. *Trans. Bot. Soc.* **34**, 55-115.
1948. *Cuneifolia* section. *Trans. Roy. Soc. Edinb.* **61**, 631-636.
1946. *Denticulata* section. *Trans. Roy. Soc. Edinb.* **61**, 469-478.
1944. *Dryadifolia* section. *Trans. Bot. Soc.* **34**, 135-148.
1943. *Farinosae* section. *Trans. Roy. Soc. Edinb.* **61**, 1-69.
1948. *Floribundae* section. *Trans. Roy. Soc. Edinb.* **61**, 637-644.
1946. *Grandis* section. *Trans. Roy. Soc. Edinb.* **61**, 467-469.
1946. *Malacoides* section. *Trans. Roy. Soc. Edinb.* **61**, 445-454.
1944. *Malvacea* section. *Trans. Bot. Soc.* **34**, 116-131.
1942. *Minutissima* section. *Trans. Bot. Soc.* **33**, 227-266.
1942. *Muscariodes* section. *Trans. Bot. Soc.* **33**, 267-294.
1942. *Nivales* section. *Trans. Roy. Soc. Edinb.* **60**, 563-627.
1946. *Obconica* section. *Trans. Roy. Soc. Edinb.* **61**, 415-430.
1948. *Parryi* section. *Trans. Roy. Soc. Edinb.* **61**, 644-650.
1944. *Petiolares* section. *Trans. Roy. Soc. Edinb.* **61**, 271-314.

1944. *Pinnatae* section. *Trans. Roy. Soc. Edinb.* **61**, 442-444.
1944. *Pycnoloba* section. *Trans. Bot. Soc.* **34**, 132-134.
1946. *Reinii* section. *Trans. Roy. Soc. Edinb.* **61**, 437-442.
1943. *Rotundifolia* section. *Trans. Bot. Soc.* **33**, 473-487.
1943. *Sikkimensis* section. *Trans. Bot. Soc.* **33**, 431-458.
1946. *Sinenses* section. *Trans. Roy. Soc. Edinb.* **61**, 430-437.
1942. *Soldanelloideae* section. *J. Linn. Soc., Lond.* **52**, 321-335.
1943. *Souliei* section. *Trans. Bot. Soc.* **33**, 459-472.
1948. *Vernales* section. *Trans. Bot. Soc.* **34**, 402-468.
1948. *Verticillatae* section. *Trans. Roy. Soc. Edinb.* **61**, 637-644.

1949. (With H. R. FLETCHER.) Additions and corrections to the genus *Primula* (up to and including 1949). *Trans. Bot. Soc.* **35**, 180-202.
1941. (With H. R. FLETCHER.) New species of Alpine primulas. *Trans. Bot. Soc.* **33**, 107-121.



Frederick Soddy

FREDERICK SODDY

Born Eastbourne 2 September 1877
Died Brighton 26 September 1956

FREDERICK SODDY was a complex personality and if we are to arrive at any degree of understanding of its aspects, I believe we have to give more than usual place to the background of his early life. It has been my conception of his life that there was really four chapters in it—

To 1900—his formative years,
From 1900 to 1911—Montreal and the disintegration theory,
From 1912 to 1918—Glasgow and isotopes,
Onwards from 1919—Oxford and his social outlook.

HIS FORMATIVE YEARS

Soddy's father was a relatively successful corn merchant who was 55 years old when Frederick was born, the seventh and last child. His father retained the inherited family tradition of deep religious feeling, consistently and regularly shown by public worship. Soddy's grandfather had aspirations to be a missionary to the South Sea Islanders and had sailed for the Antipodes only to be captured by a French privateer in the year 1798. Calvinistic sermons, to which he was compelled to listen, made a deep impression on Soddy's memories of his boyhood. These sermons were always extreme in their views and practically always contained dire threats of what might follow any tendency to leanings towards the Catholic Faith. Soddy disagreed with those views very deeply, but for understanding him it should be remembered that he was brought up in this family tradition stemming from his grandfather's time and in his own case, from his childhood. An 'evangel' of some sort was a usual and not a rare guest in the household. And so in the approach he was liable to make to things in general, I have regarded his basic method of going hard at an idea without regard to the finer feelings of others, as being to a considerable extent derived from the atmosphere of family Calvinistic outlook to which he was accustomed in his early years. Truth as it was conceived was the essential thing. The method of its presentation, even at the expense of other people's feelings, was unimportant.

In his early education he was appreciative of the successful efforts of a lady teacher who eliminated a tendency to stammer, and then it was at Eastbourne College under the guidance of a science master, R. E. Hughes, only recently down from Jesus College, Oxford, with a First Class Honours Degree, that he showed interest in chemistry and displayed a science type of mentality which had been wholly absent from the family in either of its previous or present members.

At the age of 17 he published his first chemical paper, as co-author with Hughes, following Bereton Baker on the behaviour of dry ammonia and dry carbon dioxide. The description is short and is published in the *Chemical News* of 1894 (1).* By this time he had made the acquaintance of H. C. H. (later Sir Harold) Carpenter, F.R.S. Because of his relative youth and a desire to obtain the Merton College Open Science Postmastership, it was decided that a year at Aberystwyth before going to Oxford would be helpful and by an unfortunate difficulty over Latin, this year was extended by an additional term. In the meantime he had won the 1895 Postmastership. By 1896 he was in Merton College, Oxford, renewing his acquaintance with Carpenter and was in contact with J. E. Marsh, later F.R.S., with whom Hughes had been collaborating.

From his Oxford days, first as an undergraduate and then during a postgraduate year, there are three matters worthy of note. He was active in the Oxford University Junior Scientific Society and became its Chemical Secretary. In that Society's *Journal* he published a thorough and, indeed for a piece of undergraduate work, a very remarkable paper on the life and work of Victor Meyer. The third matter to record is that he showed practical interest in organic chemistry during the year after graduating by working with Marsh on a suggested structure of camphor.

His normal academic undergraduate work came to an end in 1898 when he graduated First Class in the Honours School of Natural Science. Sir William Ramsay, with whom he was later to collaborate in an important investigation, was his external examiner—that was his first introduction to him.

His own record of the further year at Oxford after graduation is that he attempted various lines of research which did not yield any worthwhile results. At the age of 23 he considered himself equipped to apply for the Professorship of Chemistry then vacant in Toronto. With one of his characteristic decisions he decided he would follow up his application by a personal visit. His Toronto visit was fruitless for its initial objective, but he then decided to visit Montreal, attracted, I believe, by the high reputation of the laboratory equipment available at McGill University. He accepted a junior demonstratorship in the Chemistry Department at £100 per annum, but he was made economically stable by the generosity of his father. This was in May 1900 and he did not meet Rutherford until September of that year.

MONTREAL AND THE DISINTEGRATION THEORY

So far as Soddy is concerned there are no publications in 1901, but the joint Rutherford and Soddy papers start in 1902 and are essentially concerned with thorium, thorium X and thorium emanation. The collaboration between Rutherford and Soddy was a happy one. Rutherford was essentially an experimental physicist with a knowledge of chemistry that would not be regarded as fundamental. Soddy was a chemist and always had a good grasp of atomic conceptions. Although the two men were both outstandingly strong personalities and thus might easily have found it difficult to get on with one another, the fact that their approach to their own experimental work was from different directions made for ready and complementary discussion. Certainly their period of collaboration left Soddy with an appreciation of Rutherford as a great experimental physicist. The partnership in Montreal was a happy one, both from the point of view of the participants and of science.

---

* The numbers in parenthesis refer to the numbered items in the bibliography at the end of this memoir.

Re-reading the Rutherford and Soddy papers today (2), (3), (4), (5), and remembering all the time that these papers—essentially four of them—are the foundations of one of the biggest revolutions in scientific thought, I cannot help but be impressed with their matter of fact and unsensational tone. The setting out of what we know as the disintegration theory is not done with much if any headlining. Their ideas are based on two basic facts—the experimental curves for growth and decay as measured in an electroscope and the repeated assertion that the phenomena were atomic or sub-atomic and not molecular. It is difficult at this distance of time to visualize the immediate effect of these papers on the scientific world of that day. Up to then the ideas of possible explanation of radioactivity rested on a form of continuity of emission of energy analogous in some way to the wave theory of light. Continuity seemed to be the basic ideas of the Curies. The older generation of British scientists had ideas about the capacity of the radioactive elements to absorb energy from their surroundings and then to re-emit it. Harold Hartley, who by that time was very interested in the impact of the new scientific idea, describes the effect of the Rutherford and Soddy papers as that of a wave which swept over the scientific world and carried away most if not all of the previous vague alternatives. Certainly by the end of 1903 all the great names such as Lodge, Thomson, Crookes, with the signal exception of Kelvin, had acknowledged the strength of the disintegration theory. In most respects this is all very satisfactory, but I think it is unfortunate that nowhere that I am aware of does either Rutherford or Soddy give an account of the mental process by which they arrived at their conception which has so fully stood the test of time. In such conversations as I had with Soddy on the subject I could not get any closer in a definite way beyond a very specific statement that the conception was in very truth a joint effort. Kelvin remained for a year longer an unbeliever—until in fact the Cambridge Meeting of the British Association in 1904, when he acknowledged that the phenomena were atomic. But in the last year of his life (Kelvin 1907) he was writing a paper in an attempt to explain the radioactivity of radium.

Here it may be appropriate to record an incident that has come to my knowledge from the Kelvin side—from Mrs H. N. Beilby who is a grand-niece of Lord Kelvin and who was frequently in the Kelvin household in the early years of the century. After Soddy's arrival in Glasgow, Lord and

Lady Kelvin seriously discussed whether it would be desirable and wise for them to invite this young scientific revolutionary for a weekend at Largs, and they then decided to do so. The effect was most fortunate. The discussions which took place then did not, so far as I am aware, change in any way the views held by either side, but they did have a most happy result in that both Lord Kelvin and Soddy found their respect for each other on a much higher level than hitherto.

Soddy left Montreal with the objective of working with Sir William Ramsay in London and this piece of collaboration was entirely successful, namely the proof that helium was a product of radium emanation disintegration. This piece of work (8), published in 1903 in the *Proceedings of the Royal Society*, did much to remove any lingering doubts that any one had on the basic correctness of the Rutherford and Soddy disintegration conception.

There is one period to which reference should be made because I feel it is characteristic of Soddy's vitality. While he was in London in the spring of 1904 he accepted the position of Lecturer in Physical Chemistry in Glasgow University, but before he took up the appointment he set out on a lecture tour in Western Australia. The arrangement was for a course of Extension Lectures under the auspices of London University, and dealt with electrical matters before settling down to the new and, to the then, very exciting subject of radioactivity. This tour took Soddy into mining townships in Western Australia, such as Kalgoorlie and Coolgardie as well as Perth and Fremantle. As he left Western Australia en route for Glasgow via New Zealand and the United States, he called in at Adelaide and made the acquaintance of W. H. Bragg. I have Sir Lawrence Bragg's authority for saying that the impression he created in Adelaide was most favourable.

Although the work was carried out in Glasgow it is perhaps appropriate in this place to refer to his other great contribution to the establishment of the disintegration theory, namely the proof of the growth of radium from uranium. Available for this purpose was 50 kilos of uranyl nitrate given to him by Sir George Beilby. There is a long series of papers (11) on this subject in which he was assisted by T. D. MacKenzie and Miss A. F. R. Hitchins; the first paper of the series is dated June 1905 and the last November 1931. He found that the rate of growth of radium was slow at first but developed later at increased speed. He correctly deduced the presence before radium of a relatively stable radio element. Both the helium work in London and the radium work in Glasgow are important contributions to the development of the disintegration theory.

Glasgow and isotopes

The research work which was initiated after arrival in Glasgow in the main came from the extension of ideas inherent in the disintegration theory and some has been mentioned under that heading. With his growing reputation as a scientist it is natural that a number of research students should gather round him. Additional to the names already mentioned, there were A. J. Berry, who worked on rarefied gases; A. S. Russell whose early work was on γ-rays and who later was a contributor to the isotope development; Miss Ruth Pirret on uranium radium ratios; A. J. Cranston on the parent of actinium; H. Hyman on the atomic weight of lead from thorium minerals.

There is a further type of scientific work which he regarded as most valuable and that is the need of exposition of those newer ideas to the generality of well educated people. He inaugurated a series of free lectures on 'The interpretation of radium' which drew large audiences and were much appreciated. To the questioning critic it was sometimes a matter of wonder as to how much of the science conceptions the generality of his audience took away with them, but there is no doubt that the large number of lecture table experiments which he carried out made a lasting impression. He was a prolific lecturer and very popular.

Even at the time of the early Glasgow period he was liable to introduce into his lectures some of the extraneous ideas to which he was prone. For instance, in 1906, as President of the Röntgen Society, when giving a lecture on the straightforward subject of 'The present position of radioactivity' (75), he could not restrain his zeal for doing some missionary work for his financial ideas. The quotation 'The point we have reached is that the ideal money metal must be technically worthless'; and again 'Now let us, still in the position of visitors from outer space, survey the present economic position of man with relation to money. I have alluded to the position occupied by gold in the human life . . .' will serve to show the type and line of thought which were prominent in his mind even at this early date and which were among the main interests of his later years.

In the years at Glasgow the chemistry of the radio elements received a good deal of attention and from these studies by him and by others, two important inter-related but distinct conceptions evolved. The first was the identity of chemical properties possessed by some radio elements with others, i.e. the conception which came to be described as 'isotopes', and the second was the movement through the periodic table as a radio-active element emitted an α or a β ray. These movements were finally summed up in the statement of 'The displacement law'.

The germs of both conceptions were in being for some time before they were stated in anything like a final form with appropriate supporting evidence.

After the discovery of radio thorium in 1905 by Ramsay and Hahn a good deal of work was done on its chemistry and it was in connexion with this element that McCoy and Ross (1907) stated 'Our experiments strongly indicate that radio thorium is entirely inseparable from thorium by chemical processes.' Examples of pairs of radio elements with extremely similar chemical properties accumulated. Ionium was shown to be non-separable from radium; mesothorium from thorium X and radium D from lead. So far as Soddy is concerned, an important stage in the concept was reached when he realized the close relation between mesothorium and radium. That was in 1910 and two quotations may be made from his writing of that year—'When it is considered what a powerful means radioactive methods of measurement afford . . . and the completeness and persistence of some of the attempts at separation . . . the conclusion is scarcely to be resisted that we have in these examples no mere chemical analogies but chemical identities.' Again—'The recognition that elements of different atomic weight may possess identical chemical properties seems destined to have its most important application in the region of the inactive elements. . . . Chemical homogeneity is no longer a guarantee that any supposed element is not a mixture of several of different atomic weight or that any atomic weight is not merely a mean number.'

Such quotations can show that the idea of elements of different atomic weights having same chemical properties was one which took a long while to germinate and to be examined exhaustively. His paper delivered in 1922—'The origins of the conception of isotopes' (56) is a complete statement of all the thoughts and work that led up to the formulation of the Displacement Law. Otto Hahn (1957) in a recent publication, has paid an impressive tribute to Soddy in this connexion—'I had never succeeded in isolating mesothorium or thorium X from radium. . . . Those negative attempts led me to think that there was still greater chemical similarity between these groups of elements than was known to exist between the various so-called rare earths. I should never have had the courage to declare that these were chemically identical elements. But Frederick Soddy possessed that courage. He created the expression "isotopes" . . . . Thus suddenly all the difficulties disappeared. It is not the atomic weight of an element, but its nuclear charge which determines its place in the "periodic table".'

It was towards the end of 1911 that Soddy asked me to examine systematically as many as possible of the radio elements whose chemical characteristics had not then been determined. The series of results were published in 1912 and 1913 and included uranium X and thorium, radio actinium and thorium and, what was probably most important of all, they established the identity of thorium β and lead. Corresponding results showed that other radio elements were identical in chemical properties with bismuth and thallium.

It was a thorough examination of these results which called forth and justified the identity declaration referred to above by Hahn.

Up to 1913 we used the phrase 'radio elements chemically non-separable' and at that time the word isotope was suggested in a drawing-room discussion with Dr Margaret Todd in the home of Soddy's father-in-law, Sir George Beilby. Dr Todd was an Edinburgh trained medical doctor and was a writer of some distinction under the name of Graham Travers. The readiness and etymological accuracy with which she produced 'isotopes' is a standing testimony to her practical knowledge of the Greek tongue.

In 1922 Soddy published a very full paper in Stockholm on the occasion of his receiving the Nobel Prize and in it he describes his part, both in the evolution of the conception of isotopes, and also how he and others contributed to the fitting of the radio-elements into the periodic law. In this paper full justice is done to the work and ideas of Strömholm and Svedberg, of Russell, of Hevesy and of Fajans. In 1911 Soddy had written 'The loss of a helium atom or α particle appears to cause the change of the radio element not into the next family but into the next but one . . .' This was known as 'The α-ray generalization'. I believe A. S. Russell first made the suggestion that when a β ray is emitted the atom changes in chemical nature so as to pass into the family in the periodic table next higher in number.

With the characterization of the chemical properties, particularly of those radio elements subsequent to the emanation the full chemical behaviour of the three series of radio elements was made clear. Soddy was most clear that on the theoretical side the names of Fajans, Hevesy and Russell are associated in the advance which resulted in the formulation of the displacement law. It can now be stated in the following terms—Each α ray expelled causes a shift of two places in the direction of diminishing mass and diminishing valence, and each β ray expelled causes a shift of one place in the opposite direction.

At the same time when it was originally formulated it was not possible to connect in the actinium series with the uranium series. That was made possible by the independent work of Soddy and Cranston (35) (*Proc. Roy. Soc.* 1918) and Hahn and Meitner (*Phys. Z.* 1918) which showed that action was produced by an α ray change from 'ekatantalium'—an element occupying the place between uranium and thorium and, therefore, isotopic with uranium-X2.

An important piece of research work was carried out arising from the general conception of isotopes in collaboration with H. Hyman (32). The displacement law led to the conclusion that isotopes of lead were the end products of both the uranium and thorium disintegration series, and if lead should be the end of the thorium series it should have an atomic weight of 208 where the disintegration of uranium would give lead of 206:—the accepted value of common lead being 207·2. In Soddy's work he commenced with 30 kilos of Ceylon thorite and the atomic weight was determined to be 207·7.

The isotope work vastly enhanced his reputation in the scientific world and the word 'isotopes' has come to be closely associated with his name throughout the whole scientific world. It was not his first attempt to bring in a new word into scientific use. Rutherford and he had suggested that the word 'metabolon' should be used for an element possessing the properties of radioactive disintegration, but the word never found favour in the minds of scientists—it seemed just as easy to say 'radioactive elements' and even its parents ceased to use it after a few papers of the 1903 vintage.

Carrying on the process of imaginative insight into what the disintegration theory had achieved, Soddy's thoughts went on to examine some of the things that radioactivity in the fullness of time might achieve. It is, therefore, worth while recording a paper on 'The internal energy of elements' (47), read in

March 1906 to the Glasgow Local Section of the Institution of Electrical Engineers: 'We cannot regard the existence of this large internal energy as a peculiar property of the element radium. . . . Yet uranium since it produces radium with evolution of energy must possess all the internal energy of radium and more . . .' 'But there is no saying now that we are assured we are on the right track, how or when a discovery may not be made which will unlock this great store of energy bound up in the structure of the element . . .' 'We are starting the twentieth century with the prize in full view . . .'

He was a long way ahead of experimental achievement, but no one can deny that there was vision in his appreciation of the then situation and its potentialities.

Soddy's translation to the Professorship of Chemistry in Aberdeen and the outbreak of the First World War in 1914 were interruptions to his outlook—in fact so serious that apart from the collaboration with Cranston on the parent of actinium, no new experimental work on radioactivity came from him that should be recorded in this memoir.

During the 1914-1918 war he was active in various forms of chemical work which was immediately necessary for national purposes. Organic preparations were carried out in the University laboratory and semi-technical work on the extraction of ethylene from coal gas was carried out in the Aberdeen Gas Works. By the courtesy of Mrs Muriel Howorth, to whom all his papers have been left by his will, I have seen the reports of his work for the Marine Sub-Committee of the Board of Inventions and Research under whose auspices he did work on steam raising by addition of water to strong or anhydrous caustic soda. It is worth while stressing, therefore, that he was active in technical work arising from the war situation, but none of it needs to be recorded here in detail.

Oxford and his social outlook

I had almost written 'Oxford and fermenting causes' since from 1918 onwards his mind was more and more occupied with ideas analysis and suggestions as to what was wrong with the world at large, or 'why so far the progress of science has proved as much a curse as a blessing to humanity'. His own words are—'My main conclusion . . . was that it was entirely due to the fictitious money system which arose contemporaneously with the birth of the scientific civilization and that now was being purposefully and consciously used to frustrate it and to preserve the earlier civilizations founded on slavery.'

At the end of the war he was appointed Lees Professor at Oxford in which appointment he remained for some 17 years. Although he did an amount of work in the laboratory he did not publish any papers based on experimental chemistry. On that aspect of science he had shot his bolt. He spent a great deal of time on teaching aspects of his appointment, such as plans for the reconstruction of the laboratory. At this time, too, much thought was also put into machines to assist in the interpretation of physical phenomena. For instance, Mr Brewer of Oxford informs me of a machine devised to give rulings automatically increasing to correspond with the continuously varying spectrographic dispersion.

It must be recorded that neither at Oxford any more than at Glasgow and Aberdeen was he a successful teacher, although at Aberdeen he is remembered as an inspiring figure to those senior students with whom he could come into personal contact. His mental processes were different from those of the ordinary run of students so that the latter could not easily follow the words with which he clothed his thoughts. I have already mentioned the tendency he had to go out on various tracks with fervent missionary zeal. The weaknesses he saw in gold as a monetary method was mentioned in 1906. During the women's suffrage agitations of the 1910-1914 period he was more and more on the side of the militants and when he went to Aberdeen he voiced views that seemed to many to lead him into the ranks of socialism. He was, however, an individualist and it was not long before his abhorrence of anything savouring of collectivism was made manifest. The basis of his thought for the whole of his life after coming to Oxford was an endeavour to resolve the weaknesses of our modern civilized life, particularly having in mind the effectiveness of our modern thought and experiment applied to material affairs. The conclusion already mentioned gave him the impulse to a great amount of study and writing. To most of us these writings have little attraction and the best that can be said of them is that they represented an attempt to base a monetary system on an assessment of energy quantities. To an extent, therefore, there was something in common with the Canadian Social Credit ideas. However 'Cartesian economics' (60), 'The wrecking of a scientific age' (76) and other publications of a like nature did not attract many to agree with him. He continued to write and think on these lines until the 1950's but the impact of these writings tended to diminish.

While in Oxford he very frequently found himself in acrimonious discussions with fellow members of the teaching body and so for the period 1919 to 1936, there is nothing that can be recorded of a progressive nature so far as his professional life is concerned, although in his private life I know of many happy times on social parties and on the river.

In the intervals between such writings he spent some endeavours on mathematical problems and ideas and a number of papers were published in *Nature*, e.g. 'The bowl of integers' and 'the Hextet' (40) and (41) in 1937. Another example is 'The summation of infinite harmonic series (42), in the Royal Society's *Proceedings*, 1942. That type of work was far away from the grand manner of the disintegration theory and, so far as I am aware, carried little or no conviction among scientists professionally concerned with these matters.

It is, therefore, pleasant to turn away from controversial matter and to conclude this memoir by reference to some of his happier personal characteristics. He could be greatly impressed by the people in his immediate neighbourhood. Thus for so long as she lived his wife, Winifred Moller Beilby, whom he married in 1908, was a great influence with him and he enjoyed thoroughly her companionship and her company. While he was away climbing in the Alps with her he used to write delightful letters to other members of the family, giving vivid descriptions of their activities and the scenery. When she became ill his thought and care for her were unremitting and he continued to scheme domestic arrangements which might make things easier for her. Her sudden death in 1936 from a thrombosis was a great shock to him.

He was a great traveller. His first step into radioactivity arose from a quick decision, unusual in the context, to cross the Atlantic to enquire for himself into personal matters. He was in Glasgow again in 1901 for the British Association Meeting and when he had to go to Australia he arranged matters so that en route he saw something of Ceylon, the New Zealand mountains, Honolulu, the Grand Canyon and the St Louis 1904 Exposition. And so after his wife's death he decided to further the interest he had had in thorium minerals, such as monazite sands, by an extended journey to India, Malaya and Ceylon. This journey extended from November 1937 to April 1938 and was productive of letters to the Beilby family which were full of interest and personality. One letter, too long to quote, tells of a journey from Bhamo by river to Mogok and its ruby mines with a diversion to a place where an ex-Cambridge law student was carrying out the burial rites of his father, preparatory to being installed as the Ruling Sawbwa or chieftain. This place, Mongmeit, is in the North Shan States and his description of the ceremonies attended by a thousand or so of the local populace show Soddy, not as a Calvinistic missionary, but rather as a receptive young man interested in all humanity.

Mention must also be made of his generosity, particularly to young people. Russell (1956) in his memoir records that one of his last acts was to visit Oxford and give handsome cheques to laboratory stewards and assistants who had served him twenty and more years ago. I, too, can vouch for his desire to go out of his way to give facilities for advancement in a material and an educational sense.

In the latter years of his life he was very interested in the activities of the 'Le Play' Society. He journeyed with them on numerous excursions to study, as I understand it, regional social problems. By his will a Soddy Trust has been established to assist the carrying out of research in the same type of interests.

This leads to a tribute to his capacity for leadership exercised, unfortunately, on far too few occasions. He just could not make any personal contact or impact on any large body of students. To those few with whom he could work in close understanding, he was a live and inspiring leader. He was willing to devote unrestricted time to ensure that the subject under consideration was adequately discussed and thoroughly understood. When the course of action was settled then he showed those great qualities of leadership by continuing to be an inspiration, but never by asking about mere details and so giving the impression of wanting to interfere.

Soddy described himself as a professional chemist given to writing on chemistry matters within his knowledge. Remembering his writings and thoughts on economic affairs, it is not surprising that he strayed over the boundaries of both fields and was liable to pen various philosophical aspects of his outlook on life in general. A volume of his addresses in *Science and Life*, published in 1920, shows his regard for the more spiritual aspects of the things of life. Particularly there stands out his great belief in the grand concept of a university and all it stands for. He had many hard things to say about education and some even harder about educationalists, but he never lost sight of the importance of the universities and of the need for sustaining and developing the university spirit. He chose as the subject of his closing address to the Scientific Association of Aberdeen University the title 'The ideals of a science school' (74) and his final words on that occasion have to me at least a ring of greatness about them:

'Were all the powers of darkness in dominion over her yet is the University a holy place where year by year congregate pilgrims in the greatness and generosity of youth "to learn what none may teach, to seek what none may reach" to perpetuate the vision of youth after youth itself has sped. When this ceases to be true then, and then only, will the ancient Universities have grown old.'

He was elected F.R.S. in 1910.
He was awarded the Cannizzaro Prize, Rome 1913.
He was Nobel Laureate in Chemistry 1921.
He was made LL.D. Glasgow University 1934.
He was a foreign member of the Swedish, Italian and Russian Academies of Science.

Alexander Fleck

SELECTED BIBLIOGRAPHY

(Prepared from that arranged by Frederick Soddy himself)

Scientific papers

(1) 1894. (With R. E. Hughes.) The action of dried ammonia on dried carbon dioxide gas. *Chem. News*, **22**, 111.
(2) 1902. (With E. Rutherford.) The radioactivity of thorium compounds, I and II. *Trans. Chem. Soc.* **81**, 322 and 837. Reprinted in *Chem. News*, No. 2219 *et seq.*
(3) 1902. (With E. Rutherford.) The cause and nature of radioactivity, I and II. *Phil. Mag.* (6), **4**, 370 and 569.
(4) 1903. (With E. Rutherford.) Radioactivity of uranium, *Phil. Mag.* (6), **5**, 441.
(5) 1903. (With E. Rutherford.) A comparative study of radioactivity of radium and thorium, *Phil. Mag.* (6), **5**, 446.
(6) 1903. (With E. Rutherford.) Condensation of the radioactive emanations, *Phil. Mag.* (6), **5**, 561.
(7) 1903. (With E. Rutherford.) Radioactive change. *Phil. Mag.* (6), **5**, 576.

(8) 1903. (With Sir Wm. Ramsay.) Experiments in radioactivity and the production of helium from radium. *Proc. Roy. Soc.* **72**, 204.

(9) 1903. (With Sir Wm. Ramsay.) Further experiments on the production of helium from radium, *Proc. Roy. Soc.* **72**, 346, and *Smithsonian Report*, p. 203, Washington, U.S.A.

(10) 1902. The radioactivity of uranium. *Trans. Chem. Soc.* **81**, 860.

(11) 1905-31. Collected papers on production of Ra from U in *Phil. Mag.* under various titles.

I. 1905, (6), **9**, 768.
II. (With T. D. Mackenzie.) Aug. 1907, (6), **14**, 272.
III. 1908, (6), **16**, 632.
IV. 1909, (6), **18**, 846.
V. 1910, (6), **20**, 340.
VI. (With Miss A. F. R. Hitchins.) 1915, (6), **30**, 209.
VII. 1919, (6), **38**, 483.
VIII. (With A. F. R. Hitchens.) 1924, (6), **47**, 1148. (Containing period of Io and ratio of Io to Th in Colorado carnotite and Joachimstahl pitchblende.)
IX. 1931, (7), **12**, 939.

(12) 1907. Calcium as an absorbent of gases for the production of high vacua and spectroscopic research, *Proc. Roy. Soc.* A, **78**, 429.

(13) 1908. Die Wehnelt-Kathode im hochgradien Vakuum. *Phys. Z.* **9**, 8.

(14) 1908. (With T. D. Mackenzie.) The electric discharge in monatomic gases. *Proc. Roy. Soc.* A, **80**, 92.

(15) 1907. (With Baron Rudolph von Hirsch.) A gas generated by aluminium electrodes. *Phil. Mag.* **14**, 779.

(16) 1908. Attempts to detect the production of helium from the primary radio-elements. *Phil. Mag.* **16**, 513.

(17) 1909. Die Bildung von Helium aus Uranium. *Phys. Z.* **10**, 41.

(18) 1909. Multiple atomic disintegration. *Phil. Mag.* **18**, 739.

(19) 1908. The charge carried by the α-particle. *Ion*, **1**, 4.

(20) 1909-10. The rays and product of uranium X, I and II. *Phil. Mag.* (6), **18**, 858; (6), **20**, 342.

(21) 1910-11. (With A. J. Berry.) The conduction of heat through rarefied gases. *Proc. Roy. Soc.* A, **83**, 254 (Part 1); 576 (Part II).

(22) 1909. (With A. S. Russell.) The γ-rays of uranium and radium. *Phil. Mag.* (6), **18**, 620.

(23) 1911. (With A. S. Russell.) The γ-rays of thorium and actinium. *Phil. Mag.* (6), **21**, 130.

(24) 1910. (With A. S. Russell.) The constant of uranium X. *Phil. Mag.* (6), **19**, 847.

(25) 1910. (With (Mrs) W. M. Soddy and A. S. Russell.) The question of the homogeneity of the γ-rays. *Phil. Mag.* (6), **19**, 725.

(26) 1910-11. (With (Miss) Ruth Pirret.) The ratio between uranium and radium in minerals. I and II. *Phil. Mag.* (6), **20**, 345 (I); (6), **21**, 652 (II).

(27) 1910. Essais pour evaluer le periode de l'ionium. *Le Radium*, **7**, 295.

(28) 1911. The chemistry of mesothorium. *Trans. Chem. Soc.* **99**, 72.

(29) 1913. The radio-elements and the periodic law. *Chem. News*, **107**, 97; *Jb. Radioak.* **10**, 193. (Compare also *Nature, Lond.* **91**, 57; **92**, 331, 399 and 452.)

(30) 1913. The origin of actinium, *Nature, Lond.* **91**, 634.

(31) 1914. The existence of uranium Y. *Phil. Mag.* (6), **37**, 215.

(32) 1914. (With Henry Hyman.) The atomic weight of lead from Ceylon thorite. *Trans. Chem. Soc.* **105**, 1402; compare also *Nature, Lond.* **94**, 615; *Engineering*, 28.v.1915 and 1.x.1915; *Ann. Rep. Chem. Soc.* 1916, 247; O. Hönigschmid, *Z. Elektrochem.* 1919, **25**, 91.

(33) 1915. The density of lead from Ceylon thorite, *Nature, Lond.* **94**, 615.
(34) 1917. The separation of isotopes. A comment on the paper by T. W. Richards and N. F. Hall. *J. Amer. Chem. Soc.* **39**, 1614.
(35) 1918. (With J. A. CRANSTON, and in part with MISS HITCHENS.) The parent of actinium. *Proc. Roy. Soc.* A, **94**, 384.
(36) 1921. Contribution to discussion on isotopes. *Proc. Roy. Soc.* A, **99**, 97.
(37) 1934. Contribution to discussion on heavy hydrogen. *Proc. Roy. Soc.* A, **144**, 11.
(38) 1932. The α-rays of ionium. *Nature, Lond.* **130**, 364.
(39) 1936. The kiss precise. *Nature, Lond.* **137**, 1021.
(40) 1936. The hextet. *Nature, Lond.* **138**, 958.
(41) 1937. The bowl of integers and the hextet. *Nature, Lond.* **139**, 77, 154, 193, 251.
(42) 1942. The summation of infinite harmonic series. *Proc. Roy. Soc.* A, **179**, 377.
(43) 1943. The three infinite harmonic series and their sums (with topical reference to the Newton & Leibniz series for π). *Proc. Roy. Soc.* A, **182**, 113.
(44) 1904-20. *Report on radioactivity.* Annual Reports of the Progress of Chemistry, issued by the Chemical Society of London.
(45) 1903. Some recent advances in radioactivity. *Contemporary Review*, May. p. 708.

Lectures, Addresses and Obituary Notices, etc.

(46) 1904. The evolution of matter as revealed by the radioactive elements. The Wilde Lecture VIII. *Mem. & Proc. Manchester Lit. & Phil. Soc.* 16 Mar.
(47) 1906. The internal energy of elements. Institution of Electrical Engineers, Glasgow Local Section. 13.III.1906; *J. Instn. elect. Engrs.* **37**, part 179.
(48) 1906. The nature of the α-ray, *J. Röntgen Soc.* July.
(49) 1906. The evolution of the elements. Section A. British Assn. Meeting at York, 1906. *Rep. Brit. Ass.* p. 122.
(50) 1908. The source of radioactive energy (Watt Anniversary Lecture for 1908, of the Greenock Philosophical Society 17.1.1908).
(51) 1917. The evolution of matter. *Aberdeen University Review*, Feb. 1917.
(52) 1912. The origin of radium (Royal Institution, London, Lecture 15.III.1912.)
(53) 1917. The complexity of the chemical elements (Royal Institution, London, Lecture, 18.V.1917).
(54) 1919. The conception of the chemical element as enlarged by the study of radioactive change. *Trans. Chem. Soc.* **115** (delivered 19.XII.1918).
(55) 1922. Isotopes. (Institut International de Chimie Solvay. Introduction to Discussion on Atomic Structure, Reunion de Bruxelles April 1922.)
(56) 1922. The origins of the conception of isotopes (Nobel Lecture, Stockholm 12.XII.1922).
(57) 1923. The origin of the conception of isotopes (Royal Institution Lecture, London, 1.V.1923).
(58) 1939. James Ernest Marsh, *Obit. Not. Roy. Soc.* **2**, 549.
(59) 1953. Just fifty years ago. (Address to the Inst. of Atomic Information to the Layman on the occasion of the Jubilee of the Interpretation of Radioactivity at Londonderry House, 28.II.1953.)
(60) 1921. Cartesian economics. The bearing of physical science on state stewardship. (Two Lectures to the Student Unions of Birkbeck College and the London School of Economics. Nov. 10 and 17, 1921.)
(61) 1924. The inversion of science and a scheme of scientific reformation. (Based on lectures delivered at various University centres through 1923.)
(62) 1927. The impact of science on an old civilization. (Inaugural Address to the Students of the University College of Wales, Aberystwyth. 19.X.1927.)
(63) 1934. A physical theory of money. (Paper to the Liverpool Engineering Society, 31.X.1934.) *Trans. Liverpool Engrs. Soc.* **56**.

(64) 1947. The evil genius of the modern world. *Management & human relations in industry.* Vol. I. Industrial Relations Publishing Cor., 1165 Broadway, New York 1, N.Y., U.S.A.

(65) 1947. An independent scientist's views on the economic and political possibilities of atomic energy for the future. (Luncheon Address to The British Constitutional Research Association, 32 Shaftesbury Avenue, London, W.1. 30.X.1947.)

(66) 1904. *Radioactivity. An elementary treatise from the standpoint of the disintegration theory.* The Electrician Publishing Co. German Translation, J. A. Barth, Leipzig.

(67) 1909. The interpretation of radium. *Popular lectures at Glasgow University.* John Murray, 1909. Fourth Edition revised and enlarged 1920, reprinted 1922. German Translation, J. A. Barth. Translated also into Hungarian.

(68) 1910. Translation of *Brownian movement & molecular reality*, by Jean Perrin. Taylor & Francis 1910. (Italian and Russian.)

(69) 1911. Chemistry of the radio-elements. *Monographs on inorganic & physical chemistry.* Longmans Green & Co.

(70) 1914. Chemistry of the radio-elements. Part II. 1914. German Translation J. A. Barth, 1912 and 1914; French Translation, Gauthier-Villars, Paris 1915.

(71) 1912. *Matter & energy.* Home University Library. Williams & Norgate. (Taken over by O.U. Press.)

(72) 1932. *The interpretation of the atom.* Part I, *The radioactive elements & isotopes*; Part II, *The general progress of atomic chemistry;* John Murray.

(73) 1947. The story of atomic energy. *Engineering*, Part I, 3.X.1947. **164**, No. 4262, p. 313; and fortnightly after; Nova Atlantis Publishing Co. Ltd. 1949.

(74) 1920. Science & life. *Aberdeen Addresses.* John Murray. (Appendices A, B and C refer to criticism of the financial operations of the Carnegie Trust for the Universities of Scotland.)

(75) 1906. The present position of radioactivity. *Journal of the Röntgen Society.* Feb. 1906.

(76) 1927. The wrecking of a scientific age. (Terminal Lecture to the students of the University of South Wales and Monmouthshire. Cardiff. 22 Jan. 1927.)

REFERENCES

Kelvin, Lord (1907) Radioactivity of radium. *Phil. Mag.* (6), **13**, 313.

McCoy & Ross (1907) *J. Amer. Chem. Soc.* **29**, 1709.

Hahn, O. (1957) Science & the economic order. *Metallgesellschaft*, p. 97.

Russell, A. S. (1956) *Chem. Ind.* 1 Dec.

L. F. Spath

LEONARD FRANK SPATH

1882-1957

Leonard Frank Spath was born on 20 October 1882. The writer has been unable to obtain any reliable information regarding his parentage, place of birth (which there are reasons to think was overseas), and school education. A statement published elsewhere, from hearsay, that he came from South Africa has not been confirmed. It seems not to have occurred to Spath that such information would interest posterity, and he deposited no personal record with the Royal Society; nor, apparently, did he ever speak of his earlier years to friends or even in his own family circle. It is probable that he lost both parents while young.

At about the time when he first became known to members of the staff of the British Museum (Natural History) Spath occupied a studio in Chelsea, and visitors formed the impression that he was professionally engaged in some form of commercial art and had been well trained for it. A desire to study geology, he later told others, had been fostered by a keenness for mountaineering when a young man; and so he decided to become qualified for a geological career. He seems to have pursued his studies largely by private reading, but registered at Birkbeck College, London, for the Intermediate B.Sc. course in Geology for the 1908-1909 session, and attended evening classes at that institution intermittently for the next year or two. He took his B.Sc. degree in December, 1912, gaining First Class Honours in Geology. For examination purposes he specialized in the petrological side of the subject, although already more interested in fossils than in rocks.

Earlier in the same year, through the instrumentality of J. W. Evans, then in charge of the teaching of geology at Birkbeck College, Spath took part in an expedition to Tunisia, organized by a commercial undertaking for the purpose of mineral prospecting. It gave him the opportunity to make a collection of Jurassic fossil invertebrates acquired in 1913 by the British Museum (Natural History). The ammonites of this collection were described in his first paper, read in 1913 to the Geological Society of London. Soon after reading it he left on a further expedition, this time to Newfoundland, where he found few opportunities for fossil collecting. He does not appear to have gone overseas on any subsequent occasion.

Spath, who was accustomed to consult the library there, was given temporary employment in the Department of Geology of the British Museum (Natural History) in August, 1912, at first assisting to curate the collections of fossil brachiopods. Already, however, ammonites were his main interest, and at the Museum he was encouraged in their study by W. D. Lang, who was then engaged in intensive collecting from the Lower Lias of the Dorset coast. From Newfoundland Spath wrote to Lang in April, 1913, 'I wish the British Museum would employ me for a few years to work on nothing but Lyme ammonites.' On a second spell of work in the Museum (March-July, 1914), however, he was still nominally occupied with brachiopods, but from September 1914 to March 1915 he was at last employed to work at ammonites. His second paper was read to the Geological Society in May 1915. Both this and its predecessor do not convey the impression that they were the work of a beginner, but rather that their author already possessed an extensive knowledge of ammonites and their literature.

Spath married Florence Elizabeth Sweet in 1915 and had two sons, who survive him. His call-up for military service in 1916 resulted in an interruption of his scientific work at a time when he was impatient to qualify for his Doctorate. He served with the British Expeditionary Force in France and Belgium as a private in the Middlesex Regiment.

At the time of his demobilization in 1919 Spath was offered an established post on the staff of the British Museum (Natural History), but on terms which he considered unfair in view of his age (already 37) and experience, and he declined the offer. He resumed, however, part-time employment at the Museum. Although this meant that his remuneration was restricted by official regulations to a miserable pittance, he was left with ample time for other activities, notably the resumption of work for his Doctorate. In 1920 he was also appointed a temporary Demonstrator in the Geology Department at Birkbeck College; from time to time, moreover, the Geological Survey made use of his services for the identification of ammonites or on special investigations connected with their study. No doubt these arrangements were considered at first to be merely temporary, but they were destined to become permanent. For the remainder of his active life he worked in the mornings at the Museum (where he had charge of the collections of fossil Cephalopoda), taught at Birkbeck College (where he was ultimately made a temporary Lecturer) two or three evenings a week, and spent most afternoons writing up papers in his home not far from the Museum. There is no doubt that, had he chosen, he would have had no difficulty after a few years in obtaining a senior teaching post at a university, but he had little interest in self advancement. He is said, in fact, to have declined the offer of such a post at a Canadian university because it offered no facilities for research on ammonites. At London University he obtained his M.Sc. degree in 1920 and his D.Sc. in the following year.

Spath was now a recognized authority on ammonites. Material from many parts of the world began to be submitted to him for description and large monographs, together with many shorter papers, began to flow from his pen. His industry was amazing; his output of original work has been exceeded by that of few, if any, palaeontologists. His first major work, a *Monograph of the Ammonoidea of the Gault*, commenced publication by the Palaeontographical

Society in 1923. The complete list of his publications is in the annexed bibliography. Outstanding among them, in addition to the work just mentioned, are his voluminous *Revision of the Jurassic cephalopod fauna of Cutch* (1927-1933), his 'Catalogues', published by the British Museum (Natural History), of the *Ammonoidea of the Trias* (1934, 1951) and of the *Liassic family Liparoceratidae* (1938), and his series of monographs (1930-1952) describing Mesozoic invertebrate faunas from Greenland. It is noteworthy that his researches were not confined to the ammonites of any one system, as those of many other specialists in the group had been; he was equally familiar with the ammonites of the Triassic, Jurassic, and Cretaceous systems, and also had a good knowledge of their Palaeozoic predecessors.

When Spath began his researches, S. S. Buckman had recently promulgated his 'principle of dissimilar faunas' as a basis for the interpretation of sequences of Jurassic rocks throughout the world. The use of fossils for the identification of sedimentary formations had, of course, been known since the days of William Smith, but even workers on ammonites had thought that there were limits to the precision with which rocks could be correlated and dated by fossil evidence. Buckman's teachings, however, rested on the assumption that ammonite species evolved, attained a world-wide distribution, and then died out so rapidly that the possibilities of ever-increasing precision in the universal correlation of Mesozoic rocks were virtually limitless. He had divided each of the recognized stages of Jurassic time into numerous so-called 'hemerae', each marking the period of acme of development of some particular ammonite species and a very much smaller unit of time than the equivalent of the zones previously recognized. If any such species was not to be found in any area it could, he taught, be concluded that strata belonging to its hemera were missing from the area; that is, that there was a gap or 'non-sequence' in the succession.

This, therefore, was the position when the mantle long worn by the now aged Buckman as the leading British authority on ammonites was donned by Spath. How far he could go with Buckman (who had given him friendly encouragement when he started his work) was a problem that constantly exercised his mind, and his views on the subject changed from time to time as his experience grew. If the study of ammonites was to be applied to correlation on the detailed scale that Buckman insisted was possible, it would at any rate be necessary to work with very accurately collected specimens; and there was a suspicion that some of Buckman's schemes had been based on material not altogether reliable in this respect. Spath was fortunate in having available W. D. Lang's extensive collections, to which reference has already been made, and he himself collected material bed by bed from the Lias of Skye and Pabay, the Gault of Folkestone, the Neocomian of Speeton, and other formations.

Spath's descriptive work was thus accompanied by the constant publication of revised schemes for the zonal classification of the strata from which his ammonites had been collected. In 1923 he proposed a new subdivision of

Albian time into six Buckmanian 'ages' and of the strata deposited during that time into 19 zones. In 1924, following his investigations at Speeton, he divided Neocomian time into 11 'ages' during which 38 zones were laid down; and in the same year he proposed to subdivide the time of deposition of Oppel's *Bucklandi*-zone of the Lower Lias into three 'ages' and 13 hemerae. It is not possible here to trace in any detail the history of Spath's ideas on zonal classification, as they became modified from time to time. A meeting of the Geological Society in 1928 was the occasion for what appeared to be a recantation when he remarked—'The *Pettos* "subzone", for example, of Charmouth may be at a level different from that at which this ammonite happens to be at some other locality. The range of *Coeloceras pettos* in one place or another, or the period of its maximum development, that is, its "hemera", is known to correspond to the time taken in the deposition of not only the *Pettos* subzone, but eight or nine more of the "subzones" previously enumerated.' Nevertheless, as late as 1942 (by which time the Buckmanian 'ages' had disappeared from his scheme and he had virtually reverted to Oppel's original zones), Spath considered that the Lias could be subdivided into as many as 49 subzones. This scheme was offered as an approach towards 'an ideal zonal table . . . capable of being used by workers in any part of the world', to serve as a 'suitable framework for future investigators.' In most of Spath's papers he took the opportunity to review contemporaneous ammonite faunas from many parts of the world and to give his opinion as to how they fitted into the general scheme. Such reviews, which included many previously unpublished observations, are of great value. The discussions and zonal schemes in his monumental work on the Jurassic cephalopod fauna of Cutch are a case in point.

When Spath started his work on ammonites the 'theory of recapitulation' formed part of every palaeontologist's creed, and papers by Hyatt, notably his 'Genesis of the Arietidae' (1889), were acclaimed as inspired works which demonstrated conclusively the main lines of evolution of the shell-bearing cephalopods. Spath wrote in 1936—'I was so to speak brought up on Hyatt. Twenty-five years ago there was not a paper by Hyatt that I did not know, almost by heart, and, of course, implicitly accepted . . .' Spath's early paper (1914) on 'The development of *Tragophylloceras loscombi* (J. Sowerby)' was on strict Hyattian lines. The ammonite was carefully dissected, the changes shown by successive suture-lines observed, and, on the assumption that these showed recapitulation, deductions were made as to the ancestry of *Tragophylloceras* and related forms, and an improved classification of the group of families to which the Phylloceratidae belong was suggested.

As his experience broadened, however, Spath began to suspect that all was not well with current theory. Hyatt, in his eagerness to demonstrate recapitulation, had paid insufficient attention to the order in which the forms he had discussed made their appearance in the fossil record, and his ancestral forms were first met with, only too often, in later beds than their theoretical descendants. Spath's own researches, notably those on Lang's large collection of liparoceratid ammonites from the Dorset Lias, led to the conclusion that the real evolutionary order was often almost exactly the reverse of that deduced from Hyattian principles. New characters first made their appearance in younger stages of ammonite ontogeny and, as evolution proceeded, spread on to later whorls. The detailed evidence for this conclusion was presented in Spath's *Catalogue of the Liparoceratidae* (1938), although he had announced his views on the subject on several previous occasions. The general question of the evolution of the Cephalopoda formed the subject of two important papers (1933, 1936). Here, again, Spath considered that the evidence was opposed to the views of Hyatt and his followers, who, one gathers, had selected as the basis of their discussions observations which were entirely unrepresentative, and had then employed a fallacious theory to interpret them. Spath could not agree with Hyatt that the ancestral cephalopod was an orthocone (straight shell), but thought that both orthocones and nautilicones (coiled shells) developed independently from a primitive cyrtocone (curved shell).

Among Spath's outstanding qualities were a very subtle appreciation of form and a retentive visual memory. Both may have been intensified by his early training as an artist. The first enabled him to appreciate differences in ammonites scarcely definable in words, and the second enabled him, when examining a specimen, quickly to recollect the most closely comparable figure in the literature. No visitor, submitting some fragment or distorted mould of an ammonite to him for his opinion, could fail to be impressed by the keen and critical glance bestowed upon it, the moment's thought followed by a quick reference to some published figure, and the pronouncement of the name and exact geological age of the specimen. Here was a man thoroughly conversant with his subject.

Spath was often criticized for a lack of lucidity in his writings, and many certainly do not make light reading. One reason is that he assumed that his readers would possess a knowledge of the literature and of ammonites themselves as extensive as his own. At an early date it seemed obvious to him that, if the far-reaching results hoped for were to be obtained from the intensive study of ammonites, it would be necessary to have a much more precise nomenclature than then existed; in other words, that many more generic and specific names were necessary. These he strove zealously to provide; and so, in a paper on some particular formation and area there would be many digressions in which new names would be assigned to figures of specimens, probably from other regions, which had been thought inseparable from species already recognized. No author had previously founded so many new genera and species in so few words. Unfortunately this procedure gave rise to protests in some quarters, and Spath could claim to be largely responsible for the clause which was eventually added to Article 25 of the International Rules of Zoological Nomenclature requiring, from 1931 onwards, the validation of any new genus or species by a verbal diagnosis.

At Birkbeck College Spath took all the palaeontology lectures and practical work and also gave lectures on Mesozoic stratigraphy. He took half-day field classes to various localities and on rare occasions assisted with longer field courses. He was most patient in helping students, who were impressed by his integrity and thoroughness. He was also always ready to extend a helping hand to visitors from overseas, some of whom (themselves now leading authorities on fossil Cephalopoda) came to England even from across the Atlantic especially to consult him.

Misfortune dealt Spath heavy blows, for some of those dearest to him became stricken with severe maladies. These saddened his home life and restricted social activities for which, in any case, he perhaps had little inclination. All adversities he endured with fortitude, finding consolation in his work. His wife, whose health for several years had given constant cause for anxiety, died in 1942. He fought valiantly against his own failing eyesight and general health, but was obliged to give up his teaching work after carrying on for the 1951-1952 academic year. He continued for a year or two longer at the Museum, and would make special efforts to come in to meet visitors from overseas. At last, however, his attendances became less frequent, until eventually he was removed to the nursing home where his death occurred on 2 March 1957.

Spath's work was recognized by the Geological Society of London by the award of the Murchison Fund in 1924 and the Lyell Medal in 1945. He was elected to the Fellowship of the Royal Society in 1940, and in 1951 was made a Member of the Royal Danish Academy of Sciences and Letters—an appropriate honour in view of his extensive researches on the Danish collections of Greenland fossils. He was also a Vice-President of the Palaeontographical Society.

L. R. Cox.

BIBLIOGRAPHY

1913. On Jurassic ammonites from Jebel Zaghuan (Tunisia). *Quart. J. Geol. Soc. Lond.* **69**, 540-580, pls. 52, 53.

1914. On the development of *Tragophylloceras loscombi* (J. Sowerby). *Quart. J. Geol. Soc. Lond.* **70**, 336-362, pls. 48-50.

1915. On *Schlotheimia Greenoughi*, J. Sowerby sp. *Geol. Mag., Lond.* (6), **2**, 97-102, pl. 4.

1919. Notes on ammonites. *Geol. Mag., Lond.* (6), **6**, 27-35, 65-71, 115-122, 170-177, 220-225.

1920. The ammonite siphuncle. *Geol. Mag., Lond.* **57**, 142-144.

1920. On Jurassic ammonites from East Africa, collected by Prof. J. W. Gregory. *Geol. Mag., Lond.* **57**, 311-320, pl. 5; 351-362.

1920. On a new ammonite genus (*Dayiceras*) from the Lias of Charmouth. *Geol. Mag., Lond.* **57**, 538-543, pl. 15.

1921. On Cretaceous Cephalopoda from Zululand. *Ann. S. Afr. Mus.* **12**, 217-321, pls. 19-26.

1921. On ammonites from Spitzbergen. *Geol. Mag., Lond.* **58**, 297-305, 347-356.

1921. On Upper Cretaceous Ammonoidea from Pondoland. *Ann. Durban Mus.* **3**, 39-57, pls. 6-7.

1922. On Cretaceous Ammonoidea from Angola, collected by Prof. J. W. Gregory, D.Sc., F.R.S. *Trans. Roy. Soc. Edinb.* **53** (1), 91-160, pls. 1-4.

1922. On Lower Lias ammonites from Skye. *Geol. Mag., Lond.* **59**, 170-176.

1922. Upper Lias successions near Ilminster, Somerset. In Spath, Pringle, Templeman and Buckman, 'The Upper Lias Succession'. *Quart. J. Geol. Soc. Lond.* **78**, 449-450.

1922. On the Senonian ammonite fauna of Pondoland. *Trans. Roy. Soc. S. Afr.* **10**, 113-147, pls. 5-9.

1922. On the Liassic succession of Pabay, Inner Hebrides. *Geol. Mag., Lond.* **59**, 548-551.

1923. Correlation of the Ibex and Jamesoni zones of the Lower Lias. *Geol. Mag., Lond.* **60**, 6-11.

1923. Excursion to Folkestone. With notes on the zones of the Gault. *Proc. Geol. Ass., Lond.* **34**, 70-76.

1923. The ammonites of the Shales-with-Beef. *Quart. J. Geol. Soc. Lond.* **79**, 66-88.

1923. On the ammonite horizons of the Gault and contiguous deposits. Appendix 2 in *Summary of Progress for 1922, Geol. Surv. U.K.* 139-149.

1923. On ammonites from New Zealand. Appendix to C. T. Trechmann: 'The Jurassic of New Zealand'. *Quart. J. Geol. Soc. Lond.* **79**, 286-312, pls. 12-18.

1923-43. Monograph of the Ammonoidea of the Gault. *Pal. Soc. Lond.* Pp. x, 787.

1924. On the ammonites of the Speeton Clay and the sub-divisions of the Neocomian. *Geol. Mag., Lond.* **61**, 73-89.

1924. New Speeton ammonites. *Naturalist*, pp. 173-174.

1924. The ammonites of the Blue Lias. *Proc. Geol. Ass., Lond.* **35**, 188-211, pl. 18.

1924. On the Blake Collection of ammonites from Kachh, India. *Mem. Geol. Surv. India, Pal. Indica*, N.S. 9 (1), 29 pp.

1924. On a new ammonite (*Engonoceras iris* sp.n.) from the Gault of Folkestone. *Ann. Mag. Nat. Hist.* (9), **14**, 504-508.

1925. On Senonian Ammonoidea from Jamaica. *Geol. Mag., Lond.* **62**, 28-32, pl. 1.

1925. On a collection of fossils and rocks from Somaliland made by Messrs. Wyllie and Smellie. Ammonites and Aptychi. *Monogr. Geol. Dept. Hunterian Mus. Glasgow Univ.* 1, 111-164, pls. 15, 16.

1925. Notes on the ammonites of the Lower Greensand and Gault, Folkestone and neighbourhood. In J. W. Walton, *Folkestone and country around*, 31-36.

1925. Notes on Yorkshire ammonites. Part 1. On the Genus *Oxynoticeras*, Hyatt. *Naturalist*, pp. 107-112. Pt. 2. On a new Deroceratid. *Naturalist*, pp. 137-141, text-figs. 1-2. Pt. 3. On the 'Armatus Zone'. *Naturalist*, pp. 167-172. Pt. 4. On some Schlotheimidae. *Naturalist*, pp. 201-206.

1925. On Upper Albian Ammonoidea from Portuguese East Africa: with an appendix on Upper Cretaceous ammonites from Maputoland. *Ann. Transvaal Mus.* **11**, 179-200, pls. 28-37.

1925. Jurassic Cephalopoda from Madagascar. *Bull. Amer. Paleont.* **2** (44), 1-30, pl. 1.

1925. Sur quelques ammonites du Gault, nommées par P. Reynès. *Ann. Mus. Hist. nat. Marseille*, **20**, 97-106, pl. 4.

1925. Notes on Yorkshire ammonites. Pt. 5. *Arietites*, *Asteroceras* and allied genera. *Naturalist*, pp. 263-269. Pt. 6. On *Ammonites planicosta*, J. Sowerby. *Naturalist*, pp. 299-306. Pt. 7. On *Ammonites semicostatus*, Young and Bird. *Naturalist*, pp. 327-331. Pt. 8. More Lower Liassic forms. *Naturalist*, pp. 359-364.

1926. On new ammonites from the English Chalk. *Geol. Mag., Lond.* **63**, 77-83.

1926. Notes on Yorkshire ammonites. Pt. 8. More Lower Liassic forms. (2). *Naturalist*, pp. 45-49. (3). *Naturalist*, pp. 137-140. (4). *Naturalist*, pp. 169-171.

1926. Note on two ammonites from the Gin Gin Chalk. *J. Roy. Soc. West. Austr.* **12**, 53-55.

1926. Ammonites from the Black Marl of Black Ven and Stonebarrow. *Quart. J. Geol. Soc. Lond.* **82**, 165-179, pls. 9-11.

1926. On the zones of the Cenomanian and the uppermost Albian. *Proc. Geol. Ass., Lond.* **37** (4), 420-432.

1926. Notes on Yorkshire ammonites. Pt. 10. On some post-Liassic ammonites and a new species of *Bonarellia*. *Naturalist*, pp. 321-326.

1927-33. Revision of the Jurassic cephalopod fauna of Kachh (Cutch). *Mem. Geol. Surv. India, Pal. Indica*, N.S.9 (2), 945 pp., 130 pls.

1927. On Eotriassic ammonites from East Greenland. *Geol. Mag., Lond.* **64**, 474-475.

1927. On the classification of the Tertiary Nautili. *Ann. Mag. Nat. Hist.* (9), **20**, 424-428.

1928. The ammonites from the Belemnite Marls. (In Lang, Spath, Cox and Wood, 'The Belemnite Marls of Charmouth; a series in the Lias of the Dorset Coast'.) *Quart. J. Geol. Soc. Lond.* **84**, 222-232, pls. 16, 17.

1928. The Albian Ammonoidea of Nigeria. Appendix to The Nigerian Coalfield. *Geol. Surv. Nigeria Bull.* **12**, 51-54.

1928. The recent landslide at Windy Corner, above Rocker End in the Isle of Wight. *Abs. Proc. Geol. Soc. Lond., 1928-1929*, pp. 2-3.

1929. Corrections of cephalopod nomenclature. *Naturalist*, pp. 269-271.

1929. *Ammonites williamsoni* Phillips, and some allied forms. *Naturalist*, 293-298.

1930. The Jurassic ammonite faunas of the neighbourhood of Mombasa. *Monogr. Geol. Dept. Hunterian Mus. Glasgow Univ.* **4**, 13-71, pls. 1-8.

1930. On some Ammonoidea from the Lower Greensand. *Ann. Mag. Nat. Hist.* (10) **5**, 417-464, pls. 14-17.

1930. On the Cephalopoda of the Uitenhage Beds. *Ann. S. Afr. Mus.* **28**, 131-157, pls. 13-15.

1930. The fossil fauna of the Samana Range and some neighbouring areas. The Lower Cretaceous Ammonoidea: With notes on Albian Cephalopoda from Hazara. *Mem. Geol. Surv. India, Pal. Indica*, N.S. **15** (5), 50-66, pls. 8-9.

1930. The Eotriassic invertebrate fauna of East Greenland. *Medd. om Grønland*, **83**, 90 pp., 12 pls.

1931. On the contemporaneity of certain ammonite beds in England and France. *Geol. Mag., Lond.* **68**, 182-186.

1932. The invertebrate faunas of the Bathonian-Callovian deposits of Jameson Land (East Greenland). *Medd. om Grønland*, **87** (7), 158 pp. 26 pls.

1933. The evolution of the Cephalopoda. *Biol. Rev. Camb.* **8**, 418-462.

1934. *Catalogue of the fossil Cephalopoda in the British Museum* (*Natural History*). *Pt.* 4: *The Ammonoidea of the Trias.* Pp. xvi, 531, 18 pls.

1934. The Jurassic and Cretaceous ammonites and belemnites of the Attock district. *Mem. Geol. Surv. India, Pal. Indica*, **20** (4), 44 pp., 6 pls.

1934. Jurassic and Cretaceous Cephalopoda. In *Geology and Palaeontology of British Somaliland*, **2**, 205-228, pls. 24-25.

1935. Additions to the Eotriassic invertebrate fauna of East Greenland. *Medd. om Grønland*, **98** (2), 115 pp., 23 pls.

1935. On colour markings in ammonites. *Ann. Mag. Nat. Hist.* (10), **15**, 395-398, pl. 18.

1935. On the age of certain species of *Trigonia* from the Jurassic rocks of Kachh (Cutch). *Geol. Mag., Lond.* **72**, 184-189.

1935. The upper Jurassic invertebrate faunas of Cape Leslie, Milneland. I. Oxfordian and Lower Kimmeridgian. *Medd. om Grønland*, **99** (2), 82 pp., 15 pls.

1935. Field meeting at Folkestone, Kent. *Proc. Geol. Ass., Lond.* **46**, 429-431.

1935. On a Turonian ammonite (*Hamites daviesi*) from Ramri Island, Burma. *Rec. Geol. Surv. India*, **68**, 414-416, pl. 32.

1936. The Upper Jurassic invertebrate faunas of Cape Leslie, Milneland. II. Upper Kimmeridgian and Portlandian. *Medd. om Grønland*, **99** (3), 180 pp., 50 pls.

1936. On a new ammonite genus (*Sphenarpites*) from the Lias of Baluchistan. *Ann. Mag. Nat. Hist.* (10) **17**, 641-645.
1936. On Bajocian ammonites and belemnites from Eastern Persia (Iran). *Mem. Geol. Surv. India, Pal. Indica,* N.S. **22**, Mem. no. 3, 21 pp., 1 pl.
1936. So-called *Salterella* from the Cambrian of Australia. *Geol. Mag., Lond.* **73**, 433-440.
1936. The ammonites of the Green Ammonite Beds of Dorset. *Quart. J. Geol. Soc. Lond.* **92**, 438-455, pl. 33.
1936. The phylogeny of the Cephalopoda. *Palaeont. Z.* **18**, 156-181, pl. 9.
1937. Note on the phylogeny of fossil Cephalopoda. *Biol. Rev. Camb.* **12**, 154-155.
1937. The Canadian ammonite genus *Gastroplites* in the English Gault. *Ann. Mag. Nat. Hist.* (10), **19**, 257-260.
1937. The nomenclature of some Lower Chalk ammonites. *Geol. Mag., Lond.* **74**, 277-281.
1937. Mesozoic Ammonoidea. *Fortschr. Paläont.* **1**, 187-196.
1938. Problems of ammonite nomenclature. 1. The type of the ammonite genus *Mortoniceras,* Meek. *Geol. Mag., Lond.* **75**, 234-235. 2, Schlotheim's types of *Ammonites capricornus. Geol. Mag., Lond.* **75**, 362-366.
1938. *Catalogue of the ammonites of the Liassic family Liparoceratidae in the British Museum (Natural History).* Pp. ix, 191, 26 pls.
1938. Problems of ammonite nomenclature. 3. On *Ammonites varians* J. Sowerby. *Geol. Mag., Lond.* **75**, 543-547.
1939. The Cephalopoda of the Neocomian Belemnite beds of the Salt Range. *Mem. Geol. Surv. India, Pal. Indica,* N.S. **25**, Mem. no. 1. Pp. iii, 154, 25 pls.
1939. Problems of ammonite nomenclature. 4. The vicissitudes of the genus *Rhacophyllites* Zittel. *Geol. Mag., Lond.* **76**, 77-81.
1939. The ammonites of the Upper Oxford Clay of Warboys, Hunts. *Bull. Geol. Surv. Gt. Brit.* **1**, 82-98, pls. 6, 7.
1939. On a new belemnoid (*Conoteuthis renniei*) from the Aptian of the colony of Mozambique. *Bol. Surv. Indust. Min. & Geol.* (*Ser. Geol. & Min.*), *Mem. & Com.* No. 2, 1-16.
1939. On some Tithonian ammonites from the Northern Range of Trinidad, B.W.I. *Geol. Mag., Lond.* **76**, 187-189.
1939. Problems of ammonite nomenclature. 5. On *Acanthoplites jacobi* Collet, and the Jacobi Zone of the Folkestone Sands. *Geol. Mag., Lond.* **76**, 284-287.
1939. Problems of ammonite nomenclature. 6. The genus *Pachydiscus* Zittel. *Geol. Mag., Lond.* **76**, 293-296. 7. The genera *Paraphylloceras* and *Neophylloceras* Shimizu. *Geol. Mag., Lond.* **76**, 451-454.
1939. On Jurassic Ammonites from Western Australia. *J. Roy. Soc. West. Austr.* **25**, 123-135, pls. 1, 2.
1939. Mesozoic Ammonoidea. *Fortschr. Paläont.* **2**, 203-210.
1940. On Upper Cretaceous (Maestrichtian) Ammonoidea from Western Australia. *J. Roy. Soc. West. Austr.* **26**, 41-57, pls. 1, 2.
1941. On the boundary between the Upper and Lower Cretaceous. *Geol. Mag., Lond.* **78**, 309-315.
1942. The ammonite zones of the Lias. *Geol. Mag., Lond.* **79**, 264-268.
1943. Problems of ammonite nomenclature. 8. On *Ammonites cordatus* and *Amm. serratus* J. Sowerby. *Geol. Mag., Lond.* **80**, 111-117.
1944. Problems of ammonite nomenclature. 9. The genus *Stephanoceras,* Waagen, and some allied genera. *Geol. Mag., Lond.* **81**, 230-234.
1945. Problems of ammonite nomenclature. 10. The naming of pathological specimens. *Geol. Mag., Lond.* **82**, 251-255.
1946. Preliminary notes on the Cretaceous ammonite faunas of East Greenland. *Medd. om Grønland,* **132** (4), 1-12.
1946. The type of the genus *Ammonites. Ann. Mag. Nat. Hist.* (11) **12**, 490-497.

1947. Additional observations on the invertebrates (chiefly ammonites) of the Jurassic and Cretaceous of East Greenland. *Medd. om Grønland,* **132** (3), 70 pp., 5 pls.

1949. On a collection of Divesian (Mesoxfordian) ammonites from Franconia. *Ann. Mag. Nat. Hist.* (12), **2**, 422-431.

1950. The study of ammonites in thin, median sections. *Geol. Mag., Lond.* **87**, 77-84.

1950. A new Tithonian ammonoid fauna from Kurdistan, Northern Iraq. *Bull. Brit. Mus. Nat. Hist.* **1**, 93-146, pls. 6-10.

1951. *Catalogue of fossil Cephalopoda in the British Museum* (*Nat. Hist.*). *Pt. 5, Ammonoidea of the Trias* (2). Pp. xv, 228.

1951. Preliminary notice on some Upper Cretaceous ammonite faunas from Angola. *Commun. Serv. Geol. Portugal,* **32**, 123-130.

1952. Additional observations on the invertebrates (chiefly ammonites) of the Jurassic and Cretaceous of East Greenland. II. Some Infra-Valanginian ammonites from Lindemans Fjord, Wollaston Forland. *Medd. om Grønland,* **133** (4), 40 pp., 4 pls.

1953. The Upper Cretaceous cephalopod fauna of Grahamland. *Falkland Islands Dependencies Survey, Scientific Reports* No. 3, 60 pp. 13 pls.

1956. The Liassic ammonite faunas of Stowell Park Borehole. *Bull. Geol. Surv. Gt. Brit.* **11**, 140-164, pls. 9, 10.

A. G. Tansley

ARTHUR GEORGE TANSLEY

1871-1955

When he died in 1955 Sir Arthur Tansley had fortunately acceded to a standing request from the Council of the Royal Society for biographical information and in 1953 had supplied them with a typescript of what he called 'factual biographical notes'. These were in fact a good deal better than 'notes', for they are carefully composed and constitute an excellent example of his own delightfully lucid and almost laconic style of writing. Since moreover that part of them which concerns his earliest school and university career is nearly unavailable from other directions, and contains his own characteristically objective comments upon the teachers and scientists of those days, it has seemed appropriate to derive this biography, up to the turn of the century and Tansley's migration to Cambridge in 1906, almost directly from his own notes. This is signified below by the quotation marks and the concluding bracketed initials (A.G.T.).

'Tansley was born on 15 August 1871, at 33 Regent Square, W.C., the only son of George Tansley and his wife, Amelia Lawrence. He had one sister, seven years older than himself.

'[His father] was opposed to Arthur's engaging in business, and very anxious that he should go to one of the old universities, for which George had the deepest respect and admiration. Arthur showed scientific leanings, especially towards botany, which had been stimulated by a member of the Working Men's College—a wood turner by trade—who was an excellent and enthusiastic field botanist. From the age of 12 to 15 (1883-6) Arthur was at a preparatory school at Worthing where two successive masters were keen field naturalists, and their example fostered his attraction to field botany. From 1886 to the beginning of 1889 he attended Highgate School as a day boy, but here the teaching of science—limited to perfunctory chemistry—was farcically inadequate, and he told his father that he wanted to go to some place where he could get serious instruction in science and especially in biology. On the advice of a friend George decided that Arthur should attend classes at University College London, and should afterwards go to Cambridge. Accordingly Arthur left school at the beginning of 1889 and took the classes for the Intermediate Science examination of London University at University College. Here he listened to the lectures of Ray Lankester, William Ramsey and F. W. Oliver.

'Lankester was a superb lecturer, and illustrated his expositions by rapid and admirable drawings in coloured chalks on the blackboard. Unfortunately he lectured at 1 p.m. and always continued till well after 2 o'clock, sometimes till 2.40. Most of the students had been uninterruptedly at work in lecture theatres and laboratories since 9 a.m. and badly wanted some food and relaxation. The practical work in zoology was supposed to last from two till five o'clock, lunch had to be compressed into a few minutes, and it was impossible to get through the laboratory work, let alone to do justice to it, in what was left of the afternoon—often only two hours or less—and in a thoroughly fatigued condition. So intolerable did this state of things become that Tansley wrote a petition to the Professor, which was signed by nearly the whole class, emphasizing how much they appreciated his lectures, but begging him to stop punctually at 2 o'clock. Lankester was obviously disconcerted, and said something about a "light lunch" (which was all that the students ever had!) and that he would try to stop at 2 o'clock. This he did for a few days but soon relapsed into his old habits. He was apparently quite incapable of realizing the tired state of his sorely tried students who were expected to work continuously from nine o'clock till five with only a few minutes to snatch some food in the middle of the afternoon.

'Ramsay's lectures were also good, though not so brilliant as Lankester's, but the practical work was very poor, consisting of little more than pouring one reagent into a test tube containing another and noting the formation, texture and colour, or the absence of formation, of a precipitate. Tansley was much gratified by Ramsay's warm praise of an essay on Dalton's atomic theory he wrote after hearing the Professor's excellent lecture on the subject. The physics lectures were extremely dull and dreary and practical work non-existent. The students did no experiments themselves but merely saw lecture-table demonstrations which did not always come off.

'The atmosphere in Frank Oliver's botany classes was totally different. Oliver lectured very slowly, with fairly long pauses between his sentences, but when the next sentence came it was always perfectly lucid and to the point. The result was that it was easy to take very full notes, and once written-up the note-book became an excellent textbook of elementary botany, far better than any published book available at that time. The laboratory work, most of it microscopic, shared the same atmosphere: the students were expected to do everything possible for themselves, cut and stain their own sections and make careful drawings under low and high powers of the microscope. The demonstrating was very careful and thorough, and the whole training as good of its kind as it well could be. Since botany was Tansley's main subject he took Oliver's advanced classes as well as the elementary course, and in a year and a half certainly learned more, both theoretically and practically, than he ever did at any other course in his student days.

'In October 1890 Tansley went up to Trinity College, Cambridge, and read for Part I of the Natural Sciences Tripos, taking botany, zoology, physiology and geology (the last only in his third year). Since his training at University College had been so good he learned little in botany that was new, though he enjoyed the lectures of Frank Darwin and Walter Gardiner.

During his first year he did not work very hard and was mainly absorbed in undergraduate activities of an intellectual sort, society meetings and interminable talks, often into the small hours, on the usual subjects. He was very fortunate in his year, which included a very bright, not to say brilliant, set of men: Bertrand Russell and C. P. Sanger (who died so prematurely), Oswald Sickert (a younger brother of the well-known artist), Maurice Sheldon Amos, R. C. Trevelyan, Ralph Vaughan Williams, Ralph Wedgwood and John Barren. A little senior were Crompton and Theodore Llewelyn Davies, and a little junior Edward Marsh. None of these was reading science and the wide range and variety of their interests and backgrounds made their society extraordinarily stimulating and educative in the widest sense.

'Biological scientists of Tansley's year were Ivor Lloyd Tuckett and T. E. Hodgkin, and a year junior was Walter Morley Fletcher whom Tansley got to know well and who was a prominent athlete as well as a distinguished student of physiology and medicine. Fletcher afterwards became Senior tutor at Trinity and was the first secretary of the Medical Research Council, which he did a great deal to consolidate on sound foundations. Tansley always said that he owed much more to the contemporary undergraduate society of Trinity than to any other influence at the university, most of all to Bertrand Russell, who had the most penetrating mind with which he came into contact, and who was his favourite companion in midnight talks. Tansley also became a close friend of Walter Langdon-Brown of St John's, who was afterwards Regius Professor of Physic at Cambridge—a friendship which lasted till Langdon-Brown died in 1946.

'In his second year Tansley took his academic work more seriously and was awarded a Trinity exhibition as a result of the intercollegiate examination. In his third year he added geology to his other subjects. J. E. Marr, later Professor, gave the general elementary lectures, and though a rapid speaker was so clear that it was just possible to take down enough of what he said to write a full consecutive record of the lecture if that were done immediately afterwards. Thomas McKenny Hughes was Professor of Geology, and with his kind and gracious wife took a number of his students each year at midsummer for a fortnight's stay in some neighbourhood with varied and interesting geological features. These parties, which included Newnham and Girton students, were most enjoyable. The Professor made his students work quite hard, not only in the field but in the evenings, summing up the day's work and examining fossils and rock specimens, so that there was not much time for flirtation, especially as Mrs Hughes was a wise and excellent chaperone. Chaperonage, at least of a mild kind, was taken much more seriously in the early nineties of the last century than in the middle of the twentieth. To sit or stand beside the Professor in the field and follow his demonstration of the geological features of a varied landscape was an admirable training in field geology. Tansley found that the knowledge and experience so gained was of great use in his later ecological work on natural vegetation and its relation to rocks and soils.

'In 1893 Tansley took Part I of the Natural Sciences Tripos and was placed in the first class. He afterwards discovered that he was fifth of the candidates in aggregate marks. He was a fairly good, though not a first-rate, examinee. His old teacher, Frank Oliver, had invited him, before he took his Tripos, to join him as assistant at University College London. This invitation was flattering and was accepted, though Tansley afterwards rather regretted that he had not stayed at Cambridge or gone to Germany to do some original research after taking his degree and before embarking on a career of teaching. As it was he left Cambridge after the Long Vacation of 1893 and remained as Oliver's assistant till 1907, when he took up the post of University Lecturer in Botany at Cambridge which he was offered by A. C. Seward, who had succeeded Marshall Ward as Professor of Botany on the latter's death in 1906.

'In 1893-4, his first year as a teacher, Tansley had a very strenuous time, for besides his teaching work in London he had to keep up his general knowledge of botany (and to some extent also of zoology) in order to take Part II of the Tripos at Cambridge in May and June, 1894. During this time he taught himself to read scientific German so as to be able to understand important papers in that language. For Part II botany was, of course, his main subject, with zoology as the subsidiary. He was anxious about his very poor performance in the examination in practical zoology, but fortunately he pleased the zoology examiners with an essay on biological individuality, so that they agreed to his being given a first class.

'Tansley's thirteen years' association with Oliver (who was only seven years his senior) in the Botany Department at University College was a very happy one. They not only collaborated very closely in the work of the department but were warm personal friends; and their relations with W. F. R. Weldon (who had followed Ray Lankester as Professor of Zoology) and with G. F. Fowler, the Assistant Professor, were very friendly and intimate. They were constantly in and out of each other's departments, and there was an entire absence of academic formality. In 1903 Tansley married Edith Chick who had been a student in the department and afterwards held the Quain Studentship at the College with some demonstrating duties as well as time for research. He supervised her work and they published two joint papers.

'The two most conspicuous botanical activities of the botanical department during those years were its work in palæobotany and in plant ecology. The former subject had come into prominence by the massive work of W. C. Williamson of Manchester on the coal-measure fossils. D. H. Scott, who had taught at the College in the eighties, and was now Keeper of the Jodrell Laboratory in Kew Gardens, played a notable part in re-examining sections of Williamson's fossils and interpreting them in terms of modern plant anatomy. Methods of preparing sections of these fossils preserved in calcareous nodules in the coal seams had recently been perfected, and it became possible to examine them microscopically and to recognize and describe quite minute details of their structure. Oliver and Tansley utilized this material extensively and incorporated it in their teaching of the structure and evolution of vascular plants, Oliver publishing several important papers on the subject, including his most striking discovery—that certain fern-like coal-measure plants, unlike true ferns, bore seeds, thus establishing a new class of vascular plants, the "Pteridosperms". University College became in fact the leading teaching institution where palæobotanical data were used so extensively, though A. C. Seward at Cambridge, and of course Scott at Kew, were very active in research on plant fossils. Scott gave a fine course of lectures on fossil plants at the College—one of several courses on different topics which distinguished outside botanists were invited by Oliver to deliver.

'The other botanical subject which was specially developed at the College during those years was the study of vegetation and its ecology, especially its dynamic aspects, during the early years of the new century. Oliver was particularly interested in maritime vegetation—the plants which grow on sand dunes, shingle beaches and in salt marshes, and the parts they play in building up these formations on the sea coast. Though Tansley took a considerable part in this work with students in the field, it was Oliver who initiated and inspired it; and it was due to him that our knowledge of the structure and vegetation of our coastal shingle beaches was built up and recorded in a series of published papers, eventually in collaboration with E. J. Salisbury (who followed Oliver in the Professorship and eventually became Director of Kew Gardens). This dynamic maritime vegetation was studied especially at Erquy on the coast of Brittany, and afterwards at Blakeney Point on the north coast of Norfolk, where the shingle beach formation is well represented and the development of its extensions can be studied in detail.

'In 1900-01 Tansley visited Ceylon and the Malay peninsula in company with W. H. Lang (afterwards Professor of Cryptogamic Botany at Manchester), spending about three months in each and visiting Egypt for a few weeks on the way home. It was his first introduction to the wet tropics and to desert vegetation, and though he gained very useful impressions of both, the expedition did not result in any specific research with the exception of the collection of anatomical material of the ferns *Lindsaya* and *Schizæa* and of the rare and very local *Matonia* (the last in all stages of development). These formed the subjects of papers mostly in collaboration with post-graduate students, after his return.' (A.G.T.)

The considerable interest in fern anatomy indicated by these papers led Tansley to publish during his early years in Cambridge 'The evolution of a filicinean vascular system', a series of studies in pteridophyte anatomy, which secured for him a permanent and distinguished place among comparative anatomists. Here, as in most of his research, it was the elucidation and exposition of principles that held his closest attention, and evoked his best gifts. It was very much the same where he and Mr F. F. Blackman of St John's College had collaborated in writing up together their respective lectures at

London and Cambridge upon the green Algae. These were published in the earliest numbers of the *New Phytologist* and had lasting effect upon algologists' conceptions of phylogeny in the algae. Following later collaboration in Cambridge the two distinguished authors were familiarly known to undergraduates as 'Black and Tan'.

Another lecture course which was destined to have long-lasting influence was that which he devised and gave to first-year medical students: this was finally published as *Elements of plant biology*. It differed from its predecessors in its departure from a formal presentation of current elementary botany, in favour of a presentation of a newly thought-out sequence of fundamental biological principles illustrated by suitable plant material. It laid unusual stress for that period (1922) upon the mechanistic interpretation of biological phenomena and was one of the earliest plant biological texts to incorporate detailed instructions for practical work. The book was an immediate success and undoubtedly has had lasting influence upon biological teaching. It is perhaps strange that Tansley's lectures have been ultimately so powerful, for he was not a very compelling lecturer at undergraduate level. It was the content of the lectures, not their delivery, that commanded respect. Although able students found his advanced lectures exceedingly stimulating, it was in the transference of Tansley's thought to paper that they took on their particular power to excite and satisfy.

Whilst still at University College Tansley founded a new botanical journal, which at Oliver's suggestion he called *The New Phytologist*, a title which the years have shown to be increasingly baffling to publishers, booksellers and students ignorant of Greek. He writes of its origin as follows: 'Failing to get any satisfactory promise of support from other botanists in advance he determined to go ahead with his scheme by himself and leave the new journal to take its chance of success. It began in a very humble way, being printed by a small commercial printer in a back street off the Tottenham Court Road, to save expense; and the subscription was ten shillings a year for ten issues (which were very thin). Within two years, however, *The New Phytologist* was paying its way, and after a year or two more the money sunk in it (less than £130 in all) had been recovered.' (A.G.T.) After a time the size and price of the journal were increased, its circulation rose from a meagre seventy in the first year to several hundred, the standards of printing and production improved and it acquired its present substantial international standing. Tansley continued to edit the journal until 1931 when he as Professor at Oxford 'realized that his professional obligations no longer gave him the leisure to edit the journal as efficiently as he wished' and he handed over ownership and editorship to the three botanists who still control it, with characteristic generosity and commonsense conveying also 'a sufficient working balance to meet current expenses'.

Tansley aimed to make this new journal a medium for easy communication and stimulating discussion, to provide a means of keeping non-professional as well as professional botanists in touch with active developments and with new publications, to foster critical analyses of rapidly developing fields of research and to encourage consideration of botanical education. Its pages did indeed between 1917 and 1919 see some interchange of contributions, some strongly felt and sharply worded, upon the aims and methods of university botanical teaching, and when Tansley drew the series to a close few botanical teachers were unaware of a strong tendency to replace consideration of lines of descent by interest in the activities of living organisms, and to turn from the problems of origin to problems of mechanism and process.

Many of the founder's outstanding qualities are to be read from the pages of this early venture. There he displays a considerable business acumen, a deep interest in the technique and practice of book-production and publishing, a capacity for writing and editing which was to develop remarkably, a catholicity of biological interest, and, as we have seen, concern for the direction of biological education.

After his removal to Cambridge in 1907, Tansley's interest turned more and more to plant ecology, and a light lecturing commitment left free the season from Easter to October for work in the field. During the following years he acquired a considerable knowledge of the vegetation of different parts of Great Britain and he organized and conducted many student excursions. Such parties in different years visited the Norfolk Broads, the New Forest, the Forest of Dean and the Malvern districts. We draw again upon his own biographical notes to describe the growth of those ecological interests which occupied so much of Tansley's life, and made him the mainstay of British ecology and one of its acknowledged leaders in the world. 'In 1914 he took a party in July to the south of France to study the Mediterranean vegetation and that of the lower slopes of the Maritime Alps. In 1913 he spent the whole summer in the United States, a month at Chicago as the guest of his friend Professor H. C. Cowles of that University, exploring the country round, including the famous Lake Michigan dunes, and afterwards with Professor F. E. Clements, both at Minneapolis and at Clements' bungalow on the lower slopes of Pike's Peak. August and September of that year were devoted to the second International Phytogeographical Excursion which visited Chicago and its environs, the shortgrass plains of Akron, Colorado, the Pike's Peak region of the Rocky Mountains, the desert round Salt Lake City, Mount Rainier in Washington state, the surroundings of Crater Lake in Oregon, then traversing the whole length of California, including a visit to the redwoods of Muir Woods, to the Yosemite Valley, and the Mariposa Grove of Big Trees, the desert and the Salton Sea in Southern California, and finishing at Tucson, Arizona, from which an ascent of the Santa Catalina mountains was made from the desert at their foot through intermediate sclerophyll vegetation and pine forest to the spruce forest near the summits. Two years before, in 1911, Tansley had organized the first International Phytogeographical Excursion in the British Isles, inviting nearly a dozen prominent European and American botanists interested in phytogeography, and this occupied about a month, visiting selected localities in England, Scotland and Ireland. Partly to help the foreign visitors to understand British vegetation, with which most of them were quite unacquainted, a small book, *Types of British vegetation*, was prepared, edited and largely written by Tansley. This was the first systematic account of British vegetation, as distinct from flora. It had a considerable general sale and the edition was exhausted in a very few years. It was the 1911 excursion, in which Cowles and Clements took part, that prompted them to organize the second in 1913. It was then determined to make these excursions (I.P.E.) a permanent institution, and they have since been arranged every two or three years, except of course during the European war years (1914-18 and 1939-45) and for a few years after the close of each war. The Alps, Scandinavia, Czechoslovakia and Poland, Italy, Rumania, Morocco and Algeria, and Spain, have been the scenes of these international excursions, while a central organization to ensure continuity has been established at the 'Institut Rübel' in Zürich.

'In 1904, on Tansley's initiative, a small body was formed consisting of those British botanists (about twelve) who were specially interested in British vegetation, and was called the "Central Committee for the Survey and Study of British Vegetation" (afterwards shortened to "British Vegetation Committee"). The members met two or three times a year in different centres, e.g. London, Cambridge, Manchester, Dublin, to discuss their problems, such as methods of survey and of mapping vegetation, and to describe work they had recently done. It was noteworthy that there was a full muster at every meeting though some of the members had to travel long distances to attend, and great keenness was always shown at the discussions. The immediate stimulus to this step was the recent publication by the Royal Geographical and the Scottish Geographical Societies of coloured maps based on the ordnance maps on the scale of 1 inch or half an inch to the mile, with accompanying text, of various regions of the British Isles, showing the distribution of different types of vegetation within a given area. It was the members of the British Vegetation Committee who were responsible for the production of *Types of British vegetation* in 1911. In 1913 the "Committee" gave place to the British Ecological Society (of which its members formed the first council) with membership open to anyone interested in the study of ecology at large, of both plants and animals. It was felt that the time had come for the initiation of an organization of wider scope than that of the original small "committee". At the same time the *Journal of Ecology* was founded as the organ of the new society, and this Tansley edited from 1917 to 1937. The foundation of the society (and of its journal), as of its predecessor the British Vegetation Committee, were reflections of the rapidly increasing interest in ecology during the early years of the century. In 1930 a separate *Journal of Animal Ecology*, edited by Charles Elton of the Bureau of Animal Population at Oxford, was started, since primarily zoological papers, which were increasing in number, could no longer be accommodated in the *Journal of Ecology*. Both journals were successful, though the original one maintained the larger circulation.' (A.G.T.)

During his twenty years' editorship of the *Journal* it grew in size, circulation and in esteem. He had the qualities of a great editor and intense devotion to scientific advancement. Each article submitted to him was carefully considered, and the author received from him courteous and helpful suggestions based upon a sharp penetration to the underlying argument, and his own gift for clarity of language. For many years he was editing together the *Journal of Ecology* and the *New Phytologist*, and during this time his beneficent direction affected many scores of young research workers who still remain to testify to their gratitude. Such work is extremely time-consuming, and valuable as it is, can seldom be encountered nowadays.

As with most of us, Tansley concealed in himself the potentialities of many personalities or at least of several careers. He used to remark that the Almighty could never have intended him for a botanist, if only because of the difficulty he had in remembering and recognizing different species of plants (except the commonest) in the field—a terrible handicap for an ecologist. One suspects that his comment actually disclosed a deeper disquiet, for he was and knew himself to be, without any gift for experimentation. He had on the other hand such continuing preoccupation with philosophy and processes of thought that one of his Magdalen College colleagues was constrained thereby to the epigram 'if you scratch a biologist you will find a philosopher'.

When the appalling stresses of the 1914-1918 war came upon him, they reinforced a discontent which is recognizable in the sharp exchanges of the *New Phytologist* controversy on botanical teaching, and Tansley's attention focused itself upon the field of psychology. He was greatly attracted to Freud's work before it was generally known except to specialists; he read Freud's own works with concentration and sought to assimilate the Freudian concepts with acknowledged biological principles.

The prevalence of war neuroses and the mental damage associated with catastrophic trench warfare had by this time spread through the country a desire to examine the promise of the new field of psychology, and at the end of the war Tansley felt impelled to attempt an English presentation of what he understood psychological teaching to signify in terms of biology and of application to our own daily life. He wrote *The new psychology and its relation to life* and having, after some trouble, found a publisher, who gave him a profit-sharing contract, he saw it appear in 1920. This was a further instance of his acumen in the realm of book production, for his faith in himself was justified by a success which necessitated repeated reprinting so that ultimately eleven impressions had appeared. It was translated into both German and Swedish and had a substantial American sale also.

The book was kindly received not only by the general public but also by professional psychologists and psychoanalysts of high standing, and, although it had never been intended as a text book, it was widely recommended to students beginning the subject. It is a great tribute to Tansley's clarity of mind that thirty-five years later it is acknowledged that his is one of the few books of that period upon the subject that can still be read with advantage, and that does not seriously mislead. The wide sale of his book had the unexpected consequence that men and women throughout the country, enmeshed in personal difficulties, saw a promise of help in the new teachings and wrote to the author for advice. It is evident that he must have felt much conflict between his warm-hearted desire to assist, and his implacably objective conclusion that he had inadequate training to make any such intervention. In this dilemma in the summer of 1922 he visited Freud in Vienna, and during 1923 and 1924 he transported his family to that city whilst he read and studied intensively as Freud's pupil. He developed the greatest respect and affection for Freud, and whilst not unaware of weaknesses in Freud's character, recognized him as one of the greatest scientists and humanists of the world, and one destined to have the most permanent influence upon human thought. Tansley's obituary notice of Freud for this series of Royal Society publications is a movingly sincere and exact tribute.

Tansley's preoccupation with psychology was such that in 1923 he resigned his University lectureship in botany, but this represented only a relaxation of his connexions with that subject, and he continued to edit both the *Journal of Ecology* and the *New Phytologist*. When he returned to Grantchester in 1924 he seriously considered becoming a practising analyst and indeed undertook consultation with one or two patients, without however discovering in himself the temperament for making any real success herein. About this time he published a few articles and reviews in British psychological journals.

I think it is a true estimate to say that in these years Tansley found himself in the wilderness so far at least as his relations to botanical science were concerned and especially those with British botanists. Then, as now, psychology was suspect (particularly by those likely to be disturbed by its disclosures), he had no University post, and ecology, the subject to which he had really given his heart, appeared not to have made that progress which he was convinced it merited and was destined sooner or later to achieve.

In strong antithesis to apparent disregard at home, Tansley was regarded abroad as one of the great English leaders of botanical thought, and he never ceased to command the respect of all ecologists in this country as in others. It is true that he was made President of the Botanical Section of the British Association at its Liverpool meeting in 1923, but nevertheless he then found himself at the age of 52, divorced from that field of University teaching so exactly suited to his capacity and interest. Not until the end of 1926 did he complete what Freud had forecast for him, 'the return to the mother subject', and he then accepted an invitation to apply for the vacant Sherardian Chair of Botany at Oxford. He was elected in January 1927. Indecision was abandoned and Tansley acknowledged the percipience of his third University by a whole-hearted concentration upon his new duties as Professor and Fellow of Magdalen College.

The relief from University teaching during the 1914-1918 war and then again from 1923 to 1927 gave Tansley the opportunity to write and perhaps to attempt by his books to gain those effects in the world which he knew to be his responsibility and which he then lacked sufficient University standing to attain by any other means.

On accepting the Cambridge lectureship in 1906 Tansley had bought a house and garden at Grantchester, the village near Cambridge shortly to be made famous by Rupert Brooke's well-known poem. The quiet and seclusion facilitated his writing and, as we have seen, he published *The new psychology* in 1920 (with a revised and enlarged edition in 1922), and in 1922 there also appeared his *Elements of plant biology*. In the following year he published *Practical plant ecology*, a book which more than any other secured the acceptance in schools and Universities of ecology as a necessary part of biological education, and which (in revised form and changed title) continues its usefulness still. Finally in 1926 there appeared *Aims and methods in the study of vegetation*, a composite book by several authors, which he edited in conjunction with T. F. Chipp and of which he wrote a considerable part. This book was intended primarily for the use of biologists in the Commonwealth overseas, particularly those such as foresters and agriculturalists who were faced with highly unfamiliar conditions of soil, climate and biota, and who were looking to the new techniques of ecology to indicate sound lines of approach to their tasks of applying biological principles to husbandry in new environments. The book arose from the 'British Empire Vegetation Committee', set up by the Imperial Botanical Conference of 1924 to foster survey and study of the vegetation of the Empire, and *Aims and methods in the study of vegetation* was its first substantial production. Tansley (again with his eye upon the business side of book production) arranged that it should be distributed by the Crown Agents, so that booksellers' charges were avoided. The substantial profit was intended to foster publication of handbooks upon the vegetation of various parts of the Empire: the first and only one to appear has been *Vegetation of South Africa* by R. S. Adamson, one of Tansley's closest friends and admirers.

Tansley held the chair of Botany at Oxford for ten and a half years, retiring under the age limit at the end of July 1937 with the title of Professor Emeritus. The Professorship carried with it a fellowship of Magdalen College and here Tansley lived in term-time, retaining his house at Grantchester, visiting it often at weekends and spending most of his vacations there. This was Tansley's only experience of living in College as a don; he received the most kindly welcome at Magdalen and he wrote that he was thus able to learn much more about College and University ways than would have been possible if he had taken a house in Oxford. He infused new life into the Oxford botanical department, added to its staff young scientists who would foster recent developments in the subject, and made himself responsible for a considerable programme of lectures and field-work. One of his staff has written 'He prepared and pondered lectures which were lucid, studiously integrated and an inspiration to students at all University levels, and which were especially successful at that most difficult level of all, the elementary. For advanced students it was his field classes that were uniquely valuable because personal contact was closer and wherever activity and interest were evident Tansley responded generously.' It was curious that in this environment where many able young men were beginning their research careers under direction of his staff Tansley had in all only one or two research students himself, as indeed had also been the case in Cambridge. His own scientific work was so much concerned with the assimilation, consideration and analysis of the ideas and concepts of ecology, and treated British vegetation itself from so wide a basis of experience that apparently he felt no need to enlist a team of assistants in his research. It must however be said that almost all the rising ecologists in Britain at one time or another sought his advice and this, ungrudgingly given, was penetrating and beneficial, stemming indeed from a strong, intuitive perspicacity in all ecological matters.

When the Oxford department was running smoothly Tansley contemplated writing some substantial book. He hesitated between a modern university text-book of botany (certainly needed), a history of the early development of Freudian psychology (which was then not available), and an expanded version of his earlier book *Types of British vegetation* which, although long out of print, was in such demand that it came into the 'rare book' category of the booksellers. Fortunately he accepted the advice of his friends and made choice of the last-named project. It was then that he began his monumental work *The British Islands and their vegetation* that finally appeared in 1939. The need had long been acute for reprinting or replacing *Types of British vegetation.* That was a book of 400 octavo pages; its successor contained over 900 pages of much larger size, which in its second printing occupied two volumes. This growth reflected the rapid progress of ecology in Britain in the intervening 28 years, progress to a large degree through Tansley's own inspiration. He drew not only upon his own unrivalled knowledge of British vegetation but wove together a connected account from the mass of publications which had appeared in the *Journal of Ecology* and elsewhere. He went to great pains to secure exactitude, and submitted a large part of his typescript draft to the various authorities upon different sections of his subject. These readily accepted the opportunity to repay in part their debts to Tansley for past help, and many more ecologists gladly contributed unpublished material. All were assured of a presentation at the highest level of lucidity, and of scrupulously fair assessment. For his own part Tansley showed the greatest pleasure at, and appreciation of, the willingness of scientists in every field to give him this ungrudging help.

In order that the book might prove useful to a range of readers with interests not narrowly confined to plant ecology, Tansley wrote in the 'background' of climate, geology, soils and prehistory with, in his own phrase, 'considerable amplitude'. Relatively short space was given to discussion of the 'Nature and classification of vegetation', but within this section one may discover the virtues of his precise mind and philosophic training, and one can see how he reflects the development of a school of British plant sociology quite distinct from plant sociology on the European continent, and substantially different from the over-elaborated system developed by Clements in North America. The greater part of the book was necessarily descriptive, but despite the necessity for the plant lists and tables required for precision it retained a quality of readability to a high degree. Repeatedly appreciative readers wrote of their *enjoyment* of the book, and it became rightly regarded as one of the first necessities of any serious collection of books of reference on Britain. The book was handsomely illustrated and finely produced, and secured for the author the award of the Linnean Society's Gold Medal. Its production was written of as a 'milestone in British ecology' and this it certainly was. It represented the culmination of the phase of broad initial survey and interpretation in terms of the dynamic relationships of major vegetational communities; the present direction of research is towards more intimate and more experimental study of processes and relationships. This is not to denigrate in the least the value of Tansley's great work; no one else could have accomplished it, and it must remain for a very long time to come not only our own standard work of reference but the envy of the ecologists and geographers of other countries.

The leisure given by his retirement to Grantchester in 1937 gave Tansley opportunity firstly for the labour of seeing his great book through the press, and subsequently to undertake a good deal of that public work which ultimately led to the establishment of the Nature Conservancy.

'In the summer of 1942 (that is in the middle of the second world war), the Council of the British Ecological Society appointed a committee, of which Tansley was chairman, on "Nature Conservation and Nature Reserves". It was widely recognized that after the war the need to increase the economic strength of the country would involve increased threats to its scanty remaining natural and semi-natural vegetation, so that more land could be used to extend agriculture, "improve" rough grazing and "permanent grass" by ploughing and reseeding, to make new plantations on open land, as well as to exploit subsoils close to the surface by lime, gravel and roadmetal digging and quarrying, and open-cast coal-mining, besides the extensive areas that would be needed for air-fields and other military purposes. Such activities were inevitable, and the most that could be done for permanent conservation of wild vegetation and its accompanying animals would be to arrange for the preservation of some of the best examples of different types. It was generally agreed that the safeguarding of wider tracts of unspoiled wild country from disfigurement by "development" of various kinds, so that they could continue to be enjoyed by the general public, would be by the designation of "National Parks", as distinct from much smaller "Nature Reserves" whose object was the strict reservation of samples of natural or semi-natural vegetation for scientific study, as well as the habitats of interesting communities of animals, such as wild fowl or insects. The "Nature Conservation and Nature Reserves" committee duly reported to the Council of the British Ecological Society and the report, in which these ideas were elaborated, was published in both of its journals in 1944. This was not the only effort of the kind. A "Nature Reserves Investigation Committee" was also formed from another source, reporting about the same time, and the conclusions and recommendations of the two committees were closely similar. The N.R.I.C. report had the advantage of naming a number of areas recommended as reserves, the data for which had been obtained from local regional committees, and since this was published before the B.E.S. report was ready it was possible for the latter to use the data and conclusions of the former. Both of these reports served as foundations for the subsequent work which eventually led to the establishment of the Nature Conservancy in 1949. The problem of National Parks was not dealt with by either committee, since it was quite distinct from that of Nature Reserves, and was later considered by the National Parks Committee appointed by the (then) Minister of Town and Country Planning, under the chairmanship of Sir Arthur Hobhouse, whose deliberations and report led to the establishment of the National Parks Commission presided over at first by Sir Patrick Duff.

'Tansley's chairmanship of the B.E.S. "Nature Conservation and Nature Reserves" committee in 1942 was the beginning of his work for nature conservation to which he devoted a large part of his time during the next ten years. In 1945 the Minister of Town and Country Planning appointed the Hobhouse Committee just referred to, and as an adjunct to it a "Wild Life Conservation Special Committee", of which Tansley was Vice-Chairman and acted as chairman during most of its existence from 1945 to 1947. Both of these committees published reports as Command White Papers in 1947 (Cmd. 7121 and 7122). Most of the general principles embodied in the conclusions of these reports were accepted by the Government and Parliament in 1948. In March, 1949, the Nature Conservancy was established by Royal Charter and given statutory powers in the National Parks and Access to the Countryside Act at the end of that year. It was under the aegis of a committee of the Privy Council and responsible to the Lord President. Tansley was appointed chairman of the Conservancy, a post he held till 1953 when he resigned, mainly owing to increasing deafness, though he remained an ordinary member of the body.' (A.G.T.)

It was doubtless in recognition of his work for the Conservancy as also of his scientific eminence that he was knighted at the beginning of 1950.

It should be noted that in order to heighten public sensitiveness to the urgent need for conservation, Tansley had, in the early stages of the campaign, agreed to a suggestion of the Cambridge University Press to write a semi-popular book on the subject. This appeared in 1945 as *Our heritage of wild nature*; well-written and well-produced, it had a sympathetic reception and notably succeeded in its aim.

Of other quasi-public work he was a member of the Estates Committee of the National Trust, acting as their adviser on ecological matters; and President of the Council for the Promotion of Field Studies (now the Field Studies Council), from 1947 to 1953. This was a voluntary body which maintained resident centres in different parts of the country for the field instruction in various branches of natural history and art, of sixth-form pupils, undergraduates, and independent students of natural history subjects and of painting. Mr Francis Butler, whose visionary enthusiasm had been largely responsible for the foundation of the Council, attracted Tansley's immediate sympathy, and no better President could have been found. He lent the weight of his scientific prestige and he had just published two books of great value to prospective users of the field centres. The first of these was *Introduction to plant ecology* which was his early *Practical plant ecology* rewritten so as to make a more comprehensive introduction for beginners in the study of plant communities and their relationship to the environment. The second, published in the same year (1946), was *Plant ecology and the school* written jointly with Mr E. Price Evans, an ecologist and close friend, in whose original teaching methods and field research he had long been interested.

Somewhat later he contributed to a series of handbooks specifically designed for users of field study centres, a small volume *Oaks and oakwoods* attractively illustrated with sketches by the General Editor, Dr E. A. R. Ennion.

In this period when Nature Conservancy and Field Study interests had so much of his time and attention it is not surprising that he acceded to the demand for a shorter and popular presentation of the substance of his big book on the *British Isles and their vegetation.* This appeared in 1949 under the title *Britain's green mantle.* Plants were usually referred to by their English names, there was no list of species, and the style was simple. It allowed Tansley room for discourse on several topics of general interest to the lover of the countryside, such as the activities of the Forestry Commission, the extension of ley-farming, the influence of rabbit and sheep-grazing and former systems of land usage. He was typically informed and judicial about such matters, and we have been fortunate that he recorded his opinions on them in this his last major ecological publication.

He used to describe himself as 'by nature a dilettante with a strong interest in science and philosophy, some considerable analytical and critical talent, and good powers of exposition'. No one who knew him at all well would take exception to this description except to protest that dilettantism was inexpressive of the vigorous application of his mind to the several fields that at one time or another interested him. It is of course striking that he made so deep a mark first in comparative morphology, then in popular exposition of psychology, in biological teaching and above all in ecology. He glossed over or perhaps never fully appreciated that the competence left him by his father made it possible for all the decisions which changed the direction in his career to come from inclination rather than necessity; without this for instance he could scarcely have resigned his botanical lectureship to have gone to Freud in Vienna, nor in all probability could he have given so much time as he did to editing and writing.

Tansley was a man who formed deep friendships with a limited number of folk, mostly biological scientists and often 'bright young men' in whose passionate discussion of ideas he delighted to share. There never was one of them that had quite his own capacity for objective detachment and calm integrity of appraisal, no matter how beset an issue might be with intense religious, political or patriotic feeling. It hardly needs saying that this quality was of especial value in his approach to psychology, and a superb example of his style can be seen if one consults a reply to Miss Brittain that appeared in *The Nation and the Athenæum* of 15 September 1928 under the heading 'Sexual gluttony'. It was perhaps impossible to maintain a close friendship with 'A.G.' without in some degree sharing at least respect for such objectivity of mind, and after his complete disdain of grinding axes (even intellectual ones), it is painful to recall how often quite able people allow their motives to become entangled with their judgment, and how rare his quality of detachment is.

It was possibly his detachment, economic as well as intellectual, that permitted him so consistently to assume the leadership of new projects, but it is evident that these also indicate imaginative vision, and a good streak of business ability and practicality. One recalls that he conceived and established the *New Phytologist* and had the largest part in developing the *Journal of Ecology*, that he initiated the International Phytogeographic Excursion, and played a major part in founding the British Ecological Society, the Field Studies Council and the Nature Conservancy—a fair record for one who called himself a dilettante.

In 1942 he delivered the Herbert Spencer Lecture to the University of Oxford upon 'The values of science to humanity'. In it he sought, firstly to establish the conviction that science arose and arises from man's natural curiosity and is not only or even mainly the response to economic demands for technical assistance. Secondly he strove to convey that not only is science 'indispensable to our complicated material civilization: it has also intellectual, ethical, æsthetic and ultimately spiritual values of a high order'. The failure of our educational system to recognize this latter fact he regarded as greatly handicapping the nation, not only materially but spiritually.

In his later years at Oxford and after his retirement he was an active member of the Society for Freedom in Science; he wrote several articles for the Society, and more especially he threw his powerful influence into the scales for them during the critical period of the early forties when Marxist doctrine attained the dimensions of a desperate threat to liberty of scientific thought and work.

In 1953, when he was eighty-one, Tansley published his last book *Mind and life*, whose sub-title 'An essay in simplification' in part conveys the sense that the author wished to draw together the threads of his experience, and to testify to his personal philosophy. He centred his book upon 'topics whose discussion throws light on the nature and working of the human mind and its plan in human life' and it was inevitable that biology and psychology should constitute his main lines of approach in such discussion. That this book did not have the immediate acclaim of his earlier works is not surprising; the leader was no longer in the battlefield, he was retired and resuming conflicts and issues some of which had become less urgent or less interesting to the contemporary world, and perhaps also he was reaching conclusions too shrewd to be comfortable. It is none the less a work of continuing importance and interest and the most self-revealing of his writings.

Tansley was elected into the fellowship of the Royal Society in 1915 and served on its Council from 1931 to 1933, but otherwise recognition came to him rather late in his own country, even though he was regarded abroad as among the most eminent of British botanists. He derived extreme pleasure from his election to the Athenæum under Rule 2 in 1928, and at least equal pleasure from election to an Honorary Fellowship of his old college, Trinity, in 1944.

We can appreciate some of his own outstanding qualities by turning to the fine obituary notice he wrote of Sigmund Freud (*R.S. Obituary Notices* 1940), and noting how often in praising Freud, Tansley unconsciously acknowledged his own gifts. He 'showed an array of qualities very rarely combined in one man'; he 'wrote a prose style which is attractive, fluent, lucid and graceful, singularly pleasant to read and with every sentence full of meaning'; he 'was full of attractive ironic humour and with a very pungent wit', 'free from illusions about human nature', 'an uncompromising realist, but no pessimist'; he 'was indeed in love with life, though he did not fear death'. With so much and more in common, it was no wonder that he and Freud had so deep a regard for one another.

In an age when intellectuals were disastrously apt to 'shrug their shoulders and throw in their hands', Tansley conspicuously did neither, and if this was at some cost to his immediate professional career his rare courage and candour ultimately gained him both reputation and affection. It is his achievement to have substantially modified not only his chosen fields of botanical science, and to have inspired young scientists with his own ideals of objectivity and clarity, but to have modified public opinion in relation to more than one field of science and to have created some public organizations of lasting value to his country and the world.

H. Godwin

Note

The portrait which accompanies this memoir is reproduced from a colour photograph taken by Dr Eric Hultén (Head of the Botanical Department of the Swedish Natural History Museum in Stockholm) during the meeting of the International Phytogeographic Excursion in Ireland in 1949. It shows Tansley regarding one of the fen orchids in the Newbridge Fen, Co. Wicklow. I am much indebted to Dr Hultén for permission to use the picture. The cost of making the colour blocks was borne by the *New Phytologist*.

It may be noted that in his Grantchester home there is a fine portrait of Tansley painted by Edmund de Glehn, R.A., who lived in the same village during the last war, and with whom a close friendship arose. Few other portraits of him exist, although he figures recognizably in the interesting conversation piece, 'The Naturalists' by Leslie J. Watson (Royal Academy Exhibition: the 188th, 1956; No. 319) which is now in the possession of the Nature Conservancy.

SELECTED BIBLIOGRAPHY

(A list of Sir Arthur Tansley's more important publications)

A. General botany

1923. Some aspects of the present position of botany. Presidential Address to the British Association, Botanical Section, Liverpool. *Rep. Brit. Ass.* 240.

1924. The unification of pure botany. *Nature, Lond.* **113**, 85.

B. Plant anatomy

1896. The Stelar theory—a history and a criticism. *Sci. Progr.*, April-May, **5**, 133, 215.

1901. (With Edith Chick.) Notes on the conducting tissue-system in Bryophyta. *Ann. Bot.* **15**, 1.

1902. (With R. B. Lulham.) On a new type of fern-stele, and its probable phylogenetic relations. *Ann. Bot.* **16**, 157.

1903. (With Edith Chick.) On the structure of *Schizaea malaccana. Ann. Bot.* **17**, 493.

1904. (With R. B. Lulham.) The vascular system of the rhizome and leaf-trace of *Pteris aquilina* L., and *Pteris incisa*, Thunb. var. *integrifolia* Beddows. *New Phytol.* **3**, 1.

1905. (With R. B. Lulham.) A study of the vascular system of *Matonia pectinata. Ann. Bot.* **19**, 475.

1907-8. Lectures on the evolution of the filicinean vascular system. *New Phytol.* **6**, 25, 53, 109, 135, 148, 187, 219, 253; **7**, 1, 29.

C. Plant ecology and theory of vegetation

1904. The problems of ecology. *New Phytol.* **3**, 191.

1911. *Types of British vegetation.* Cambridge Univ. Press.

1917. On competition between *Galium saxatile* and *Galium sylvestre* on different types of soil. *J. Ecol.* **5**, 173.

1920. The classification of vegetation and the concept of development. *J. Ecol.* **8**, 118.

1921. (With E. J. Salisbury.) The Durmast oak-woods of the Silurian and Malvernian Strata near Malvern. *J. Ecol.* **9**, 19.

1922. Early stages of redevelopment of woody vegetation on chalk grassland. *J. Ecol.* **10**, 168.

1923. *Practical plant ecology: a guide for beginners in the field study of vegetation.* London: Allen and Unwin.

1925. (*a*) The vegetation of the southern English chalk (Obere Kreide-Formation). *Festschrift Carl Schröter. Veröff. geobot. Inst. Rübel*, **3**, 406.

1925. (*b*) (With R. S. Adamson.) The chalk grasslands of the Hampshire-Sussex border. *J. Ecol.* **13**, 177.

1926. (*a*) (With R. S. Adamson.) A preliminary survey of the chalk grasslands of the Sussex Downs. *J. Ecol.* **14**, 1.

1926. (*b*) (With T. F. Chipp.) *Aims and methods in the study of vegetation.* London: British Empire Vegetation Cttee., and Crown Agents for the Colonies.
1929. Succession: the concept and its values. *Proc. Intern. Congr. Plant Sci.*, Ithaca, 1926. **1**, 677: Menasha, Wis: Banta.
1929. (With H. Godwin.) The vegetation of Wicken Fen. *Nat. Hist. of Wicken Fen*, Part V, Cambridge: Bowes and Bowes.
1932. (With A. S. Watt.) British beechwoods. In Rübel, *Die Buchenwälder Europas, Veröff. geobot. Inst. Rübel*, **3**, 294.
1935. The use and abuse of vegetational concepts and terms. *Ecology*, **16**, 284.
1939. British ecology during the past quarter-century: the plant community and the ecosystem. *J. Ecol.* **27**, 513.
1945 *Our heritage of wild nature: a plea for organized nature conservation.* Cambridge Univ. Press.
1946. *Introduction to plant ecology*. London: Allen and Unwin.
1947. The early history of modern plant ecology in Britain. *J. Ecol.* **35**, 130.
1949. *Britain's green mantle: past, present and future.* London: Allen and Unwin.
1952. *Oaks and oakwoods.* London: Methuen.

D. Botanical education

1917. The reconstruction of elementary botanical teaching. *New Phytol.* **16**, 241.
1922. *Elements of Plant Biology.* London: Allen and Unwin.
1927. The future development and functions of the Oxford Department of Botany. An Inaugural Lecture. Oxford Univ. Press.

E. Psychology and philosophy

1920. *The new psychology and its relation to life.* London: Allen and Unwin.
1941. Sigmund Freud. *Obit. Not. Roy. Soc.* **3**, 247.
1942. *The values of science to humanity* (the Herbert Spencer Lecture to the University of Oxford, 1942). London: Allen and Unwin.
1946. (With J. R. Baker.) The course of the controversy on freedom in science. *Nature, Lond.* **158**, 574, 796.
1952. *Mind and life: an essay in simplification.* London: Allen and Unwin (translated into Swedish as *Psykologi och Livsåskådning*, 1954. Stockholm. Natur och Kultur.)

APPENDIX

Arthur Tansley's affection for his father is clearly shown in the following extract, which also illustrates the circumstances that helped to shape his own early choice of scientific career and to ensure his own remarkable liberality and integrity.

'George Tansley was a "ball and rout furnisher", a business which he inherited from his father (Samuel Tansley) and which he greatly expanded after Samuel's death till it became the leading firm of its kind in London. George (born in 1836) was taken from school by his father before he was 12 years old to help in the business, and though Samuel gave him some private instruction in certain subjects, he had no further systematic education of any kind till the age of 19. Since he had a great thirst for accurate knowledge he felt this lack very severely, and his passion for the education of which he had been deprived as a boy became the great pre-occupation of his later life. In 1855 George happened to see a notice about the Working Men's College, founded the year before by Frederick Denison Maurice, to give a liberal education to working men, as far as that was possible in evening classes. George hastened to enrol himself as a student though he was not, in any strict sense, a "working man", but his father's assistant. He had indeed grown into the main support of the business, of which he became the active proprietor after Samuel's death a few years later.

'George fastened with avidity on the opportunity provided by the W.M.C. and very rapidly learned all that the College could teach him, though he had only his evenings in which to do it. He qualified for certificates in all the subjects within the limited range available, and in the course of two years became himself a teacher at the College. At that time all the teaching was voluntary and the teachers mainly Oxford and Cambridge graduates who had been strongly attracted by Maurice's ideas; they included J. M. Ludlow, Tom Hughes, Charles Kingsley, F. J. Furnivall, R. B. Litchfield, D. G. Rosetti and John Ruskin. Afterwards they were joined by a limited number of "student teachers", of whom George Tansley was the most conspicuously successful. The close association of university and working men in the common work of teaching and learning was very fruitful in producing the spirit of fellowship for which the College became renowned. The accent was always on "liberal" education, and a favourite slogan of the College was "a man needs knowledge not only as a means of livelihood but as a means of life". There was much controversy about the permissibility of "technical" classes, a few of which were eventually admitted.

'George specialized in teaching mathematics, a field which attracted his love of accurate detail and clear thinking. His arithmetic class eventually became the most famous as well as the largest class in the College. He taught it with the utmost enthusiasm combined with the most careful attention to the difficulties of his students, who were often men without any previous education to speak of. He was never satisfied till every one of his pupils thoroughly understood every point in the work, and showed it in their "home-work". The result was that his audience were fascinated by his teaching of a subject ordinarily considered dry and mechanical, and the teacher inspired not only respect but deep affection.

'In the years after his father Samuel died, George continued to be closely occupied during the day with the development and organization of his expanding business, but he always gave all the time he could spare to the College, in which his interest was by no means confined to his own teaching, but was equally given to the government of the College and to its social life, which was a distinctive and characteristic College feature. In 1863 he married the sister of an old fellow-student. By 1884 he considered that he had made enough money to assure his family of something more than a competence and decided to retire from business completely. Successful as he had been he never liked his business: as a radical in politics he disapproved of such a "luxury trade" and was glad to leave it, and he had never cared for making money as a pursuit. From then till he died in 1902 he was able to devote practically his whole time and energy to College work, and he became by far the most conspicuous and dynamic, as well as the most greatly loved, personality in the College life.' (A.G.T.)

O. W. Tiegs.

OSCAR WERNER TIEGS

1897-1956

OSCAR WERNER TIEGS died on 5 November 1956 at the age of 59. He was at the height of his intellectual power, and in him we have lost a great biologist of this generation. His scientific work is not easily summarized, for his interests extended from physiological analysis of nervous and muscular action to studies in classical invertebrate embryology comparable to the very best work of the last century. He repeatedly turned and returned from one field of work to another. But all his work bore the highly individual stamp of the man. It had clarity, insight and thoroughness, which made his memoirs, long though they are, eminently readable. And this quality was combined with a remarkable power of apt and beautiful illustration.

It is scarcely ever safe to predict what part of a man's work will be the most enduring. But in the case of Oscar Tiegs it will surely be the major part he has played in leading us to divide the great phylum of the Arthropoda into two distinct parts; the one comprising the insects, myriapods, and that curious relic *Peripatus*, the other comprising the Trilobites, the Crustacea and the arachnids. His work did not simply make one more phylogenetic division in our animal classification. His long detailed researches have established the strongest body of circumstantial evidence for the close relationship of the members of the Myriapod-Insect group, and have thereby greatly increased our understanding of these animals.

Oscar Tiegs was born in Brisbane, Queensland, on 12 March 1897. His parents were of German origin and had migrated to Australia at an early age. His father pursued a professional career both in pharmacy and in engineering. He had a high regard for learning, and particularly for natural science, and it seems to have been his influence which first turned his son towards natural history. Oscar Tiegs recollected himself as a rather timid and industrious boy with an absorbing interest in the collection of insects. Indeed, he managed to amass a collection of some thousand named beetles from the neighbourhood of Brisbane which eventually found its way to the Queensland Museum. He gratefully acknowledged the friendship and guidance of Mr Henry Tryon, Entomologist to the Queensland Government; by some regarded as a difficult man—but pleasant, courteous and helpful to this little boy: we have reason to be grateful to him.

Till the age of 14 Oscar Tiegs attended the Brisbane State School. It was not a happy time for him and he failed to obtain one of the many Government scholarships for the Grammar School; a reminder of the unreliability of early examinations. Nevertheless, he went to the Brisbane Grammar School from 1911-1915. There he found life much more congenial and at once began to do well. On leaving school he was awarded a scholarship to the University.

The University of Queensland had only been founded eight years before, but it had acquired an excellent teaching staff. Oscar Tiegs entered the faculty of science, already with the intention of specializing in biology. This he did under T. H. Johnston, and received what may be termed a 'classical' training in animal morphology: there was no physiology.

It was during his honours course that he made his first essay in research. He made an anatomical study of a curious Echiuroid worm, *Pseudobonellia.* Characteristically, he had hit on something unusually interesting, for in this relative of *Bonellia* the males exhibited a greater degree of degeneration than in any related species. The tissues actually fuse with those of its female partner and host. After a brief excursion into helminthology he moved to the newly created Department of Zoology at Adelaide, where in the temporary absence of the Professor he had to organize the new department and its teaching.

At Adelaide he came under the influence of Wood Jones, the Professor of Anatomy, and the physiologist Brailsford Robertson, one of the pupils of Jacques Loeb and of world-wide reputation. During his three years at Adelaide he obtained the degree of D.Sc., writing for his thesis a long memoir on the histology of metamorphosis of a Chalcid wasp, *Nasonia.* This was really the foundation of much of his later work. Not only was it the forerunner of his embryological work, but it also led him to an interest in the fine structure of muscle. He found clear evidence that the apparent striation of the fibres did not arise from separate disks, but from a helicoidal organization within the fibre. He found a similar condition in certain vertebrate muscles and published a note on it in 1922. Later on he discovered that former histologists had recorded the same thing, though their observations had received little attention. He returned to this problem in his last piece of research, an exhaustive study of the flight muscles of insects and of other arthropod muscles, published in the *Philosophical Transactions* for 1955.

Oscar Tiegs was an outstanding microscopist and the beautiful preparations which he showed me personally were entirely convincing of the reality of this 'helicoidal striation' in these muscles. How far he will be justified by the future in contending that this is a very general feature of striated muscle, and how far he will be justified in having erected thereon a hypothesis that muscular conduction takes place along this helicoidal path, remains to be seen. At present, the evidence for the generality of a helicoid fine structure in striated muscle seems to be against him. But nature has a way of ingeniously choosing for its machinery the parts which come to hand: and it may well be that his latest evidence of helical conduction of slow contractile waves in the muscles of spiders and other arthropods is correct for these types. Certainly his cinematographic records seem here to support his interpretation.

But the importance of a man's work commonly rests not on his hypotheses, but upon what he does with them. It is a virtue of all Oscar Tiegs's work that his meticulous accuracy, his power of great labour, and his shrewd perception of relationships in multitudinous detail inevitably caused him to create new knowledge of lasting value whatever contemporary interpretation might be put on it. His last analysis of the comparative myology and evolution of the flight muscles of a great array of different insects is just such a piece of work. Not only does he throw a great light on how such muscles came to be evolved, but he brings sharply into prominence the importance of structural evolution at the histological level of organization. Indeed, one of the most remarkable results of his work is the demonstration that whereas in the Orthoptera and simpler insects histogenesis of muscle takes place simply by repeated division of rudimentary muscle fibres, yet in more than one of the higher orders a new principle of growth appears. Free individual myoblasts apply themselves to the young muscle fibres, each laying down a new fibril within it and making its contribution of sarcoplasm and nuclei to the whole. In his own words 'the formation of a striated muscle fibre is a more plastic process than we have supposed'.

But to return to his younger days. The influence of Brailsford Robertson on this young morphologist is, I believe, clearly to be seen. Between 1922 and 1934 he turned himself almost entirely towards the physiology of nerve and muscle. The 'lactic acid' hypotheses of muscular action of the twenties led him to search for bases which might play a part in contraction, and the experiments he published in 1925 drew his attention to the importance of creatine. This was before the discovery of phosphagen, and if he had had more contact with biochemical work current in the northern hemisphere he might have played a part in the revolution in muscle-chemistry of those times.

As a histologist, he became interested in Boeke's suggestion of a double innervation of vertebrate skeletal muscle. This led him to a physiological study of the sympathetic system in the muscle. But he obtained no evidence of sympathetic innervation there except in the blood vessels, and all the effects he obtained on muscular action seemed attributable to the production of an adrenalin-like substance at that site. Here again he passed near a later discovery. Bearing in mind his power as a histologist he might well have found himself on the trail of the small-nerve innervation later investigated by Katz and his colleagues.

This period of work was certainly profitable. It cleaned up errors and uncertainties in the work of others and showed the validity of some traditional interpretations. The same may be said of the long and thorough experiments he did with the late Professor Agar to test McDougall's supposed demonstration of Lamarckian inheritance of training in rats. The results of these experiments were negative and showed the inadequacy of McDougall's controls. That sort of work is most valuable; the removal of such errors does not always get the credit it deserves. But it so happened that in none of this work did Oscar Tiegs have the power to show his insight and novelty of mind—his power to see through a millstone—as he did in his last great work.

His physiological studies had another important consequence. They enabled him to obtain a Rockefeller Travelling Fellowship to visit Europe. There he made many valuable contacts—and many lasting friendships. In 1925 he had joined Professor Agar's department at Melbourne as Senior Lecturer, and it was in 1928 that through his Fellowship he was enabled to work for a while at Professor Wilson's Anatomical School at Cambridge, and Professor Boeke's at Utrecht. Many look back at his visit with pleasure. If there is one regret when we consider Oscar Tiegs's career it is that during some forty years of active research only twice did he visit the northern hemisphere. The loss is double: to those who found how much they had to gain from him, and to him through missing those contacts with current work in other lands which are invaluable in these days of overwhelming and rapid publication. And also there are many of us who deeply regret that plans to encourage him to visit us again on the occasion of the coming Darwin celebrations in 1958 and to receive some formal recognition of his work, were prevented by his death.

In 1933 Oscar Tiegs returned to his interest in insect metamorphosis. That was the beginning of the great work on the embryology and phylogeny of myriapods and insects to which reference has earlier been made. Such embryological work requires great labour and pertinacity. It also requires great histological skill and insight—qualities which Oscar Tiegs now possessed to the full. Clear-cut answers in this kind of research demand good fixation of difficult and varied material as well as ingenuity of perception.

He followed in full detail the development of three animals, the beetle *Calandra*, the symphylan *Hanseniella*, and *Pauropus*. He could not have chosen a better series. His original choice of *Calandra* was made to allow him to study a less profound metamorphosis than he had seen in *Nasonia*, and particularly to re-examine the question of the 'rejuvenation' of cells. He found the concept of 'rejuvenation' to be invalid, but this work led him on to study of the embryology. He showed the impossibility of reconciling the development of the midgut with a normal process of gastrulation. As Heymons had maintained for some other insects, the midgut of *Calandra* arises from the stomodaeum and proctodeum and is thus of ectodermal origin.

The thorough knowledge of the embryology of an insect obtained by this work in turn directed his attention to the light which detailed embryological studies of simpler relatives of the insects might throw on the evolution and structural relationship of the whole group. He accordingly undertook over a period of four years a thorough investigation of the embryology of the symphylan *Hanseniella agilis*, and followed it by a three-year study of the embryology of *Pauropus silvaticus*. It had frequently been suggested that such members of Myriapoda might prove to be close relatives of the immediate ancestors of the insects themselves: but there was a major difficulty. The Myriapoda seems to be an unnatural group in which community of habit has led to community of structure; for whilst some members resemble insects in that they are 'opisthogoneate', with posterior genital openings, others, including the Symphyla which are remarkably like insects in many features, are 'progoneate' with the genital opening quite differently placed, anteriorly. Either, therefore, we must reject the large clear-cut distinction between progoneate and opisthogoneate, or we must reject the many small points of resemblance between the Symphyla and the insects.

In questions of this kind there is always some tendency for the human mind to over-emphasize the importance of large clear distinctions. They are so easily apprehended and so eminently amenable to deductive logical analysis. But such large distinctions may receive some explanation which renders them less absolute; and if a very large number of small, seemingly unconnected, resemblances are shared by two animals, the circumstantial evidence in favour of their evolutionary relationship becomes quite overwhelming. Oscar Tiegs's work showed this was so in the present case.

He showed first that, contrary to expectation, the progoneate genital ducts did not arise as is usual from coelomoducts in the embryo, but secondarily as epidermal ingrowths late in larval life. The progoneate condition seems in fact to be merely a secondary adaptation to the so-called 'anamorphic' mode of growth of some myriapods by which new segments become added to the posterior end of the growing larva.

Oscar Tiegs thus reduced the major classificatory significance of the progoneate and opisthogoneate division. He also increased very greatly the number and significance of known points of embryological resemblance between these myriapods and the insects. He drew his evidence both from the myriapods and from the primitive insects *Collembola* and *Campodea*. He, thereupon proposed a new classification of these animals on the basis of head-structure. *Pauropus* and probably the Diplopoda became the Dignatha, possessing only two cephalized gnathal segments; other myriapods, together with the insects, became Trignatha, with three such segments; whilst within the last group the Symphyla, collembolids and the insects were distinguished by the possession of a labium (Labiata). Work in other fields has gone far to support his conclusion, particularly the demonstration of similar intrinsic antennal muscles in Symphyla and *Campodea*, first shown by Imms, and, very significantly, it is supported by Manton's comprehensive analysis of locomotor behaviour and machinery in these animals.

All this has placed the close relationship of insects and myriapods on a much clearer and firmer basis than before. But further, the work of both Oscar Tiegs and others has brought up once again the strength of the relation of these animals with the Onychophora. It has been a curious feature of the phylum Arthropoda that it has long suffered from what one might term a surfeit of ancestors. Had we known only of *Peripatus*, or only of the trilobites, our task might seem to have been simpler. But we are now beginning to see that the arthropod phylum itself seems cloven, perhaps down to its annelid base; and that the Onychophora-Myriapoda-Insecta stem is quite separated from the Trilobita-Crustacea-Arachnida stem. At the time of his death Oscar Tiegs left the full draft of a review on the evolution of the Arthropoda. Sadly we must regret that he was unable to see it through publication. But we are very glad to know that it was in a state in which his friends could prepare and see it through the press in the manner he would have wished. Zoologists generally will look forward eagerly to its appearance; the last touch to the work on the Myriapoda which is his monument.

In 1933 Oscar Tiegs became Associate Professor of Zoology at Melbourne. Subsequently he became Professor and succeeded as head of the Zoological Department on the death of Professor Agar in 1951. He was always primarily interested in learning and research. Like some others with that inclination, he found administration and committee work distasteful—dead sea fruit garnished with hope deferred. Official memoranda had a way of reaching the waste paper basket before their due time; perhaps with the hope that if the matter were important sooner or later someone would phone him about it. As a departmental head he gave his staff great freedom of action and encouraged their research. He was perhaps too wrapped up in his own many-sided problems to be intensely curious about the research of others unless related to his own. But he was very proud of his staff and dearly loved to bring a visitor to look around and see all they were doing.

He was always concerned to see that they were not unduly burdened with teaching and would saddle himself with extra courses during the sabbatical leave of one of his staff in order that the load on others should not be increased.

His own teaching consisted of lectures on elementary zoology to the first year students and a few special lectures on arthropod evolution and the vertebrate nervous system to senior students. His lectures were models of presentation and clarity. He did not use notes and gave his matter slowly—so that in effect he dictated an admirable text book of elementary zoology. That text book was, so to speak, a conservative one. He gave the first year student a solid background of comparative morphology. He left it to his staff to build on that in subsequent years.

His firm belief in the traditional morphological approach was also seen in the time and care he spent on improving and extending the departmental museum. That museum is now outstanding; the envy of others and a splendid memorial to him.

He was a tremendous worker, and only in later years did he begin to allow himself much leisure to cultivate interests outside his absorbing researches. But as the years went by he allowed himself more and more time to music. Beethoven was his favourite, and Mozart. He was, in fact, a man of wide æsthetic interests, which included not only music but a great love of and critical appreciation of pictures.

Simplicity, great tolerance, warm-heartedness and a tendency always to believe the best of people characterized him. He was honest and direct to the point of bluntness. He was a man who could establish lasting friendships.

Though he visited Britain so seldom, yet he had made and retained true friends. Many will remember his kindly letters during the difficult times of the last war. And many will remember with gratitude the gift parcels which he and his wife then sent to their many friends.

We have all lost in him a true friend, vivid with his bluff laugh and his openness of manner, and his moments of unexpected penetration in sizing up human character. He is a loss to biological science and to his University and to his country; a loss of a kind we can ill afford today, when so few both have the power and can spare the long time for the long and difficult fundamental problems at several removes from their technical consequences. But above all his friends will offer their sympathy to his wife, whose friendship and kindness in both great and small things they will always remember.

C. F. A. PANTIN

BIBLIOGRAPHY

1918. (With C. D. GILLIES.) On the occurrence of Pacinian corpuscles in the pancreas of the domestic cat. *Proc. Roy. Soc. Queensland*, **30**, 159.

1919. (With T. HARVEY JOHNSTON.) *Pseudobonellia*, a new echiuroid genus from the Great Barrier Reef. *Proc. Linn. Soc. N.S.W.* **44**, 213.

1920. (With T. HARVEY JOHNSTON.) A new species of *Bonellia* from Port Jackson. *Rec. Aust. Mus.* **13**, 73.

1921. (With T. HARVEY JOHNSTON.) New and little known sarcophagid flies from South-Eastern Queensland. *Proc. Roy. Soc. Queensland*, **33**, 46.

1921. (With T. HARVEY JOHNSTON.) On the biology and economic significance of the chalcid parasites of Australian sheep-maggot flies. *Proc. Roy. Soc. Queensland*, **33**, 99.

1922. (With T. HARVEY JOHNSTON.) Notes on the biology of some of the more common Queensland muscoid flies. *Proc. Roy. Soc. Queensland*, **34**, 66.

1922. (With T. HARVEY JOHNSTON.) New and known Australian sarcophagid flies. *Proc. Roy. Soc. Queensland*, **34**, 181.

1922. (With T. HARVEY JOHNSTON.) New Gyrodactyloid Trematodes from Australian fishes, together with a reclassification of the super-family Gyrodactyloidea. *Proc. Linn. Soc. N.S.W.* **47**, 83.

1922. On the arrangement of the striations of voluntary muscle fibres in double spirals. *Trans. Roy. Soc. S. Aust.* **46**, 222.

1922. Researches on the insect metamorphosis. Part I. On the structure and post-embryonic development of a Chalcid wasp, *Nasonia*. Part II. On the physiology and interpretation of the insect metamorphosis. *Trans. Roy. Soc. S. Aust.* **46**, 319.

1923. Structure and action of striated muscle fibres. *Trans. Roy. Soc. S. Aust.* **47**, 142.

1923. On the path and velocity of the excitatory impulse within striated muscle fibres. *Trans. Roy Soc. S. Aust.* **47**, 153.

1924. On the mechanism of muscular action. *Aust. J. Exp. Biol. & Med. Sci.* **1**, 11.

1924. The effect of doing work on the heat production of muscle during activity. *Aust. J. J. Exp. Biol. & Med. Sci.* **1**, 47.

1924. A colour test for guanidine bases together with some physiological applications. *Aust. J. Exp. Biol. & Med. Sci.* **1**, 93.

1924. Studies on plain muscle. *Aust. J. Exp. Biol. & Med. Sci.* **1**, 131.

1925. The function of creatine in muscular contraction. *Aust. J. Exp. Biol. & Med. Sci.* **2**, 1.

1925. The nerve net of plain muscle, and its relation to automatic rhythmic movements. *Aust. J. Exp. Biol. & Med. Sci.* **2**, 157.

1926. On the surface tension changes which underlie muscular and amoeboid movements. *Aust. J. Exp. Biol. & Med. Sci.* **3**, 1.

1926. On the inadequacy of the conception of the neurone as the unit of conduction of the nervous system. *Aust. J. Exp. Biol. & Med. Sci.* **3**, 45.

1926. The structure of the neurone junctions of the spinal cord. *Aust. J. Exp. Biol. & Med. Sci.* **3**, 69.

1926. Further remarks on the structure of the spinal cord. *Aust. J. Exp. Biol. & Med. Sci.* **3**, 161.

1927. A further note on the structure of the neurone junctions of the spinal cord. *Aust. J. Exp. Biol. & Med. Sci.* **4**, 25.

1927. The structure of the neurone junctions in sympathetic ganglia, and in the ganglia of Auerbach's Plexus. *Aust. J. Exp. Biol. & Med. Sci.* **4**, 79.

1927. A critical review of the evidence on which is based the theory of discontinuous synapses in the spinal cord. *Aust. J. Exp. Biol. & Med. Sci.* **4**, 193.

1928. Surface tension and the theory of protoplasmic movement. *Protoplasma*, **4**, 88.

1928. (With A. E. Coates.) The influence of the sympathetic nerve on skeletal muscle. *Aust. J. Exp. Biol. & Med. Sci.* **5**, 9.

1929. The microscopic structure of neurone junctions in the central nervous system. *Proc. Kon. Akad. Wet. Amst.* **32**, No. 2.

1929. On the neurofibril structure of the nerve cell. *Aust. J. Exp. Biol. & Med. Sci.* **6**, 111.

1930. (With A. E. Coates.) Further observations on certain alleged effects of sympathetic nerves on skeletal muscle. *Aust. J. Exp. Biol. & Med. Sci.* **7**, 37.

1931. (With A. E. Coates.) Are the skeletal muscles of the extremities directly innervated by sympathetic nerves? *Aust. J. Exp. Biol. & Med. Sci.* **8**, 99.

1931. (With A. E. Coates.) Sympathetic ramisection and the treatment of spastic muscle. *J. Coll. Surg. Australasia*, March.

1931. A study of the neurofibril structure of the nerve cell. *J. Comp. Neurol.* **52**, 189.

1931. A note on the posture of the bird's wing, and its supposed control by sympathetic nerves. *Amer. J. Physiol.* **98**, 547.

1931. A note on the occurrence of sarcocystis in muscle of python. *Parasitology*, **23**, 412.

1932. The innervation of the striated musculature in python. *Aust. J. Exp. Biol. & Med. Sci.* **9**, 191.

1932. A study by degeneration methods of the innervation of the muscles of a lizard (*Egernia*). *J. Anat.* **66**, 300.

1932. Nerve endings in human teeth. *J. Anat.* **66**, 622.

1933. (With A. B. Corkill.) The effect of sympathetic nerve stimulation on the power of contraction of skeletal muscle. *J. Physiol.*, **78**, 161.

1934. Observations on the structure of striated muscle fibre. *Proc. Roy. Soc.* B, **116**, 38.

1934. A cardio-depressor substance (acetyl choline?) released by dorsal nerve root stimulation. *Aust. J. Exp. Biol. & Med. Sci.* **12**, 161.

1934. The function of sympathetic nerves in relation to skeletal muscle evidence for humoral action. *Proc. Roy. Soc.* B, **116**, 351.

1935. (With Florence V. Murray.) The metamorphosis of *Calandra oryzae*. *Quart. J. Micr. Sci.* **77**, 405.

1935. (With W. E. Agar and F. H. Drummond.) First report on a test of McDougall's Lamarckian experiment on the training of rats. *J. Exp. Biol.* **12**, 191.

1938. Further remarks on the terminations of nerves in human teeth. *J. Anat.* **72**, 234.

1938. (With assistance by Florence V. Murray.) The embryonic development of *Calandra oryzae*. *Quart. J. Micr. Sci.* **80**, 159.

1939. Two new Victorian Symphyla of the genus *Manseniella. Mem. Nat. Mus. Melbourne,* No. 11.

1940. The embryology and affinities of the Symphyla, based on a study of *Hanseniella agilis. Quart. J. Micr. Sci.* **82**, 1.

1942. (With W. E. Agar and F. H. Drummond.) Second report on a test of McDougall's Lamarckian experiment on the training of rats. *J. Exp. Biol.* **19**, 158.

1942. The dorsal organ of collembolan embryos. *Quart. J. Micr. Sci.* **83**, 153.

1942. The dorsal organ of the embryo of *Campodea. Quart. J. Micr. Sci.* **84**, 35.

1943. A new species of *Pauropus* from Victoria. *Mem. Nat. Mus. Melbourne,* No. 13.

1945. The post-embryonic development of *Hanseniella agilis* (Symphyla). *Quart. J. Micr. Sci.* **85**, 191.

1947. The development and affinities of the Pauropoda, based on a study of *Pauropus silvaticus. Quart. J. Micr. Sci.* **88**, 165.

1948. (With W. E. Agar and F. H. Drummond.) Third report on a test of McDougall's Lamarckian experiment on the training of rats. *J. Exp. Biol.* **25**, 103.

1949. The problem of the origin of insects. Pres. Address Section D, *Aust. & New Zealand Ass. Adv. Sci., Hobart.*

1952. Wilfred Eade Agar. *Obit. Not. Roy. Soc.* **8**, 3.

1953. Innervation of voluntary muscle. *Physiol. Rev.* **33**, 90.

1954. (With W. E. Agar, F. H. Drummond and M. M. Gunson.) Fourth (final) report on a test of McDougall's Lamarckian experiment on the training of rats. *J. Exp. Biol.* **31**, 308.

1955. The flight muscles of insects—their anatomy and histology; with some observations on the structure of striated muscle in general. *Phil. Trans.* B, **238**, 221.

1955. (With E. Matthaei.) The path of the slow contractile wave in arthropod muscle fibre. *Phil. Trans.* B, **238**, 349.

J. S. Townsend

JOHN SEALY EDWARD TOWNSEND

1868-1957

JOHN SEALY EDWARD TOWNSEND, one of the great physicists of this century, lived through a period during which the fundamental concepts of natural science underwent a bewildering series of changes. Unlike many of his scientific colleagues, he remained completely undismayed by the storms raging in the world of physics, though he accepted some of the new ideas with due scepticism. It is often noticed that scientists when they approach the end of their active lives turn to other fields of human endeavour such as mysticism, economics or cosmogony. But this certainly did not apply to Townsend, who remained a pure physicist until the end of his life.

Townsend was born on 7 June 1868 at Galway, Ireland, the second son of Edward Townsend, Professor of Civil Engineering at Queen's College, Galway. He was educated at Corrig School and entered Trinity College, Dublin in June 1885 as a pensioner, an ordinary student, where he read mathematics, mathematical physics and experimental science. A year later he was elected to a Foundation Science Scholarship in mathematics. In 1890 he obtained a double Senior Moderatorship, being placed first in mathematics. In the same year he received the degree of B.A. During the following four years he was a Fellowship Prizeman and as such he was engaged in teaching; in particular he lectured on mathematics.

At the age of 27 Townsend went to Cambridge as an advanced student, joining Trinity College in October 1895. There, together with Rutherford, he became one of J. J. Thomson's research students, J.J. having been appointed to the Cavendish Chair in 1884. At that time Larmor, C. T. R. Wilson, McClelland and McLennan were at the Cavendish Laboratory, as well as Langevin who became a great friend of Townsend's. His brilliant mind soon became apparent. In 1898 he was made Clerk Maxwell Scholar and a year later Fellow of Trinity College Cambridge. From 1899 to 1900 he was an assistant University demonstrator in the Cavendish Laboratory.

An account of Townsend's life and work can be divided conveniently into four periods: the first covers his activities at the Cavendish Laboratory, the second his work at Oxford until the beginning of the first world war, the third encompasses his researches between the two world wars and the last period the work during his retirement.

1. TOWNSEND'S WORK IN CAMBRIDGE

At J. J. Thomson's suggestion Townsend started with a magnetic problem. (A paper on solar disturbances erroneously attributed to Townsend was published by his namesake in 1892.) His maiden work was a measurement of the susceptibility of ferric and ferrous salts in aqueous solutions. By means of a sensitive induction balance, the increase of the mutual inductance due to the solution was compensated by a known small inductance. This work engaged Townsend for only a very short time.

His next problem, however, aroused his interest in a field which occupied him during the greater part of his life. Townsend studied the electric properties of gases which are released by the electrolysis of liquids. It was known before that such gases carry electric charges and that when these are passed through a moist gas, clouds are readily formed. From his extensive experiments Townsend deduced that condensation of atmospheric clouds is likely to be due to electrified gases. He observed for example that the density of the cloud in his vessel increased with the density of the electric charge and he noted particularly that negatively charged oxygen formed larger droplets than the positively charged gas, an observation which he explained by Thomson's theory. In other experiments he proved that the formation of clouds with oxygen containing ozone is not the result of the action of electric charges. This and the following work made Townsend familiar with electrochemistry and the chemistry of gases.

Townsend's outstanding contribution in Cambridge was the first direct determination of the elementary ionic charge. It revealed his unique ability in experimental technique coupled with elegance and simplicity of method. In this work he used three instruments: a laboratory balance, an electrometer and a photographic camera. The principle of the method was as follows: By means of electrolysis a gas is produced which carries electric charges of one sign. It is passed through water of known temperature until it becomes saturated so that the mass of vapour admixed to the gas is given. The cloud formed is then driven through a series of tubes containing a drying substance and by weighing the tubes the mass of water in the cloud is found. Also, the rate of fall of the cloud in a vessel is recorded photographically and by applying Stokes's law the average mass of a single droplet is obtained. Finally, the total charge of the cloud is measured by driving it to a collector connected with an electrometer. From these observations Townsend found the ionic charge of a droplet to be about 3×10^{-10} e.s.u. Moreover, the value was very nearly the same for different gases. This remarkable method was afterwards modified by J. J. Thomson and Harold Wilson and led to Millikan's precision method with charged droplets of oil.

Later Townsend turned his attention to diffusion problems associated with electrified gases which he treated both experimentally and theoretically. In order to test the results of his theory he developed a new method to measure the diffusion of ions in gases: A stream of gas passes a chamber in which it is ionized by X-rays or otherwise. The gas flow carries the ions of both signs through several parallel thin metal tubes so that recombination and self-repulsion are negligible and only diffusion to the walls of the tubes can occur. The charge of the ions which escape from the ends of the tubes is measured with tubes of different length with a collector which repels ions of one sign. The results were striking: the coefficient of diffusion of ions in their own gas was about one-quarter of that of neutral molecules; negative ions diffused slightly faster than positive ones. This meant that the mean free path of ions in gases is considerably shorter than that of molecules.

Another interesting contribution deals with secondary X-rays. By means of a simple ionization chamber Townsend measured the intensity of secondary radiation which is emitted from various substances when irradiated by ordinary X-rays. He found that aluminium and glass were poor, copper and zinc strong secondary emitters, that soft X-rays were easily absorbed by air and that the ionization in the gas is exactly proportional to the pressure if the additional effect of secondary rays on the electrodes of the chamber is allowed for.

2. Townsend in Oxford, 1900-1918

(*a*) *Teaching activity*

In 1900 Oxford University decided to found a new Chair of Experimental Physics. Townsend, on account of his brilliant scientific record, was elected to become the first holder of the Wykeham Chair at an age of 32. It is interesting to recall the list of Electors: the Vice-Chancellor Dr T. Fowler (President of Corpus Christi), G. E. Thorley (Warden of Wadham), A. E. H. Love (Sedleian Professor of Natural Philosophy), W. Odling (Waynflete Professor of Chemistry), Sir William Huggins (President of the Royal Society) and E. H. Hayes (Fellow of New College) who was an Elector on that occasion since the Wykeham Chair carried a Fellowship at New College.

At that time the physics school was under the direction of R. B. Clifton, a London cum Cambridge man, 64 years old, who had been appointed to the Chair of Experimental Philosophy in 1865. Clifton was in charge of the old Clarendon Laboratory, where C. V. Boys has made his measurements of the gravitational constant. It was obvious that with the creation of a second chair of physics the responsibilities for teaching and research had to be redistributed between the two Professors. Thus a change of statute was made, the relevant part of which reads: 'The subject on which the Professor of Experimental Philosophy shall chiefly lecture and give instruction shall be the mechanics of solid and fluid bodies, sound, light, and heat. The subject on which the Wykeham Professor of Physics shall chiefly lecture and give instruction shall be electricity and magnetism.' The statutes also mention that the University may assign to the Wykeham Professor the duty of taking charge of a laboratory.

However, when Townsend took up his appointment he was only provided with a few rooms at the Observatory, then in the charge of H. H. Turner who was most helpful to his young colleague. Townsend obtained from the University £700 'to fitting up the laboratory and £250 per year for the first two years for assistance and maintainance'. His own emoluments were about

£500 provided by New College of which £100 were stated to be 'a further annual payment so long as the College has funds available for the purpose.'

Townsend's duties during the first years were regulated by decree. He was to lecture during two out of three terms, to reside at Oxford for at least four months during each academic year and give a minimum of two lectures a week between 1 September and 1 July extending over at least six weeks, and altogether lecture at least during 14 weeks during each academic year. Townsend's first announcement in the University Gazette read: 'The Professor will lecture on electricity and magnetism on Tuesdays and Thursdays from 10 to 11.' He delivered his inaugural lecture on 25 April 1901 at the old Clarendon Laboratory. Its title was 'Recent developments in electro-optics'. I have not been able to find a record of it anywhere.

For many years Townsend had to carry out his research in rooms lent to him by two of his colleagues, the Professor of Astronomy and the Professor of Physiology. He hoped that a new laboratory for teaching and research would be built for him, but though he received support from many quarters his plan did not meet with success. In 1902 he was offered rooms in the University Museum then occupied by the Radcliffe Library which he thought were not sufficiently large to accommodate a well-organized laboratory.

There was still much to be done on the teaching side. This is shown by a circular letter addressed by the Vice-Chancellor to all the heads of departments about the needs of the University and is dated February 1902. In it it is stated that 'previous to the foundation by New College of the Wykeham Professorship of Physics in 1900 an important part of physics—electricity and magnetism—was practically omitted from the subjects taught or studied in this University'. Townsend recognized that the teaching of undergraduates in their first year must include all parts of physics. In 1903 he took over the teaching of the preliminary course for which the University—according to Clifton's report—provided spacious rooms. This remark referred to the erection of a large tin hut (on the site now occupied by the extension of the Department of Zoology) which was given the proud name Electrical Laboratory. By that time the University had provided means for a demonstrator and other assistants. Later Townsend set up a workshop of his own and engaged G. A. Bennett as a mechanic. As time went on Bennett acted also as Townsend's glassblower, secretary, administrator and accountant.

Townsend's ceaseless efforts to obtain adequate laboratory facilities were finally crowned with success. The University Gazette records that on 20 October 1908 a decree was proposed in Convocation which stated that the Court of Assistants of the Drapers' Company of London had decided to authorize their Estate Committee to employ T. J. Jackson, R.A., as architect for the erection of a new laboratory at a cost not exceeding £22 000 for building and accessories and £1000 for furniture and equipment. The laboratory was to be a gift to the University but the University had to provide a site, the maintenance of the laboratory and the salaries for teachers and demonstrators. In November 1908 the building operations started and on 21 June 1910 the new Electrical Laboratory was presented by the Master of the Worshipful Company of Drapers to the Chancellor of the University. It was a two-storey building with stone walls up to 3 feet thick, lavishly provided with stone pillars for setting up sensitive instruments. In order to facilitate magnetic research the use of steel was avoided as far as possible. The lecture theatre could hold up to 100 people. In this laboratory Townsend worked until his retirement in 1941. He built up a flourishing school, with not more than about six research workers at a time, who gathered around him from all over the world.

(*b*) *Research activity*

Townsend's first work in Oxford constitutes the nucleus of one of his most important contributions. It was concerned with the multiplication of charges in a gas subjected to an electric field. In those early days the concept of ionization of gases was not developed to any appreciable degree. For example, it was thought that in order to ionize air an ionization energy of about 175 eV was required. Townsend derived from his own work and previous work by Stoletow that very much lower energies are sufficient to stimulate this process. In a paper which he presented to the International Electrical Congress of St Louis he states that 'it can be shown that molecules of a gas can be ionized by the impacts of ions that have acquired their velocity under electromotive forces of 10 to 20 volts'.

He was also well aware of the nature of the charged carriers which are mainly responsible for ionizing neutral atoms. This point is of some importance. Critics of his work assert that Townsend thought that the ionization of a gas is caused by negative ions and not by electrons—a term which he introduced in his writings at a later date. From a study of his original papers it transpires that in 1900 he described the negative ions as particles which carry an elementary charge and possess a mass which is small compared with that of the parent molecule. In 1901 he proved that 'negative ions thus produced (by collision) in a gas are identical with the negative ions set free from the electrode by the action of the ultra-violet light'.

Incidentally, these early papers reveal that Townsend was convinced at that time that the prime movers in the ionization of gases in electric fields are the negative ions only and that the positive ions which are simultaneously produced do not play any role in it whatsoever. In contrast, some of his colleagues in other universities firmly believed in the preponderant function of positive ions as agents of ionization. It will be shown below that Townsend acquired in later years a view which was nearly diametrically opposite to his original one which suggests that his outlook—as far as physics is concerned—was not so conservative after all.

Returning to the problem of multiplication of charges, Townsend demonstrated in a series of convincing experiments that electrons which are released from the negative plate of a plane condenser by X-rays or ultra-violet light and moved by an electric field through a gas multiply in the gas. Their number increases exponentially with the electrode separation if the field is kept constant and the separation not too large. Moreover, he showed that the proper parameter is not the field but the field reduced to unit gas pressure, a magnitude proportional to the energy which an electron acquires when it travels one mean free path in the field direction. Townsend then determined the coefficient of multiplication (later called Townsend's first ionization coefficient) which gives the number of ion pairs produced by one electron which moves 1 cm in the direction of the electric field. He found that this coefficient when reduced to unit gas pressure is a function of the 'reduced' field for each gas and proved in this way that the rule of similarity holds.

The dependence of the ionization coefficient on the field was described by Townsend in form of a semi-empirical relation. Again, it is interesting to find that he was well aware of the restricted range in which this relation was valid: from experiments with different gases he concluded that his relation not only fails at very low and very high values of the reduced field, but also fails completely for certain gases. Townsend showed that one of the constants in his semi-empirical equation was proportional to the ionization potential and he hoped originally to develop in this way a method of measuring the ionization potentials of various gases, a hope which was never fulfilled.

Previous to Townsend's work it was thought that when a gas was exposed to constant irradiation the current flowing through the gas cannot exceed a certain value whatever the magnitude of the applied potential. It came as a great surprise to scientists and engineers when Townsend claimed that he was able to produce new ions in a gas at low pressure with an electric field which was feeble compared to that necessary to initiate sparks. His experimental results which are regarded nowadays as perfectly plain and unambiguous were then strongly criticized; his theory was attacked by many physicists of repute for reasons which today are more difficult to understand than Townsend's physical ideas.

In the following years Townsend and his pupils set out to investigate the conductivity of feebly ionized gases, the motion of negative ions (electrons) and the function of positive ions, which he thought were essential in releasing secondary electrons from the cathode on which they fell. He defined a second ionization coefficient which gives the number of secondary electrons for each positive ion which hits the cathode. In other words, Townsend was looking for the cause of the fast rise of the multiplication which occurs when the electrode separation is near to the critical one at which breakdown occurs. By allowing for secondary electrons, that is by including a second ionization coefficient in his theory, he obtained an expression for the multiplication which approaches infinity for certain values of the field and electrode separation. This critical condition he took to describe the electric breakdown of the gas.

Combining this condition with his semi-empirical relation mentioned earlier, Townsend derived a relation between the sparking potential of a gas and the product of gas density and electrode separation, this product representing the number of molecules in the gas. His theoretical result was in excellent agreement with Paschen's experiments, which had shown that the sparking potential rises when the product of gas pressure and separation becomes either very large or very small having a minimum of about a few hundred volts. For voltages below this minimum value no breakdown can occur. Townsend explained this behaviour in simple physical terms: at lower pressures or small separations, that is with decreasing number of molecules in the gap, the ionization diminishes and in order to obtain multiplication to infinity the potential has to be increased. At high gas pressure on the other hand, or at large electrode separations, an increase in pressure increases the collision rate between electrons and molecules, thereby reducing the mean energy of the electrons; thus fewer fast electrons are available, the rate of ionization decreases and breakdown can only be obtained by a corresponding increase in potential.

A problem which puzzled physicists for a long time was an observation made by Stoletow. When two plane parallel electrodes are connected with a source of constant potential and a photo-electric current is emitted from the cathode it is found that as the gas pressure is increased the current first increases and then passes a maximum. Townsend applied his theory to this case and was able to explain it using arguments of the type given above.

Many of Townsend's early ideas are being taught today and thus are widely known. However, probably few know that Townsend was at one time very interested in the electro-chemistry of gases. Kirkby's studies under Townsend can probably be regarded as the beginning of quantitative investigations into the chemical changes which occur in an ionized gas. It can only be surmised that these researches came to a sudden end when Kirkby resigned from his demonstratorship on becoming the incumbent of Saham Rectory, Watton, Norfolk.

Next Townsend turned to a study of the motion of charges in gases. Here he introduced a series of revolutionary ideas. First of all he showed theoretically as well as by measurements of the lateral diffusion of electrons in gases which move in a uniform electric field, that the electron swarms behave like light foreign gas atoms which are admixed to the parent gas; but the energies of the electrons are distributed about the mean energy which in certain cases can be more than a hundred times that of the mean energy of the molecules of the parent gas. This idea appeared to his contemporaries rather obscure because at that time it was not realized that, in spite of the numerous collisions, a 'hot electron gas' can be maintained owing to the large mass difference between electrons and molecules.

Having thus demonstrated that electrons in their swarm possess very large random velocities, Townsend devised a new method of measuring the drift velocity of electrons (that is their average speed in the field direction) by observing the deflexion of the swarm when a magnetic field was applied in a direction perpendicular to the electric field. He found that the drift velocity was several orders of magnitude smaller than the random velocity of the electrons. Furthermore, he was able to derive from these experiments the fraction of energy which an electron loses, on an average, in a single collision. In a monatomic gas this fraction was found to be equal to a value which follows from kinetic theory of gases provided the field is not too strong; but as the (reduced) field is increased this fraction rises, indicating that inelastic collisions between electrons and gas molecules set in. From these experiments he even derived a quite good value for e/m, showing again that free electrons were the carriers of charge.

In another series of papers Townsend extended the diffusion theory to include the motion of ions in gases. One of the most remarkable results which originate from a theoretical treatment and which he tested extensively by experiment was the relation between ion diffusion and ion mobility. In short, he found that the ratio of the coefficient of diffusion and the mobility was inversely proportional to the gas temperature provided the ions moved in moderate electric fields.

This result has an important application. It is well known that measurements of the diffusion coefficient of ions in gases are inherently difficult because of a great many uncontrollable factors which influence the motion of charges. The above result, however, enables us to derive the coefficient of diffusion from measurements of the mobility of ions which can be carried out with a much greater accuracy.

Another result which follows from these investigations is the connexion between the charge of a gaseous ion and that of a monovalent ion in a liquid electrolyte. Townsend showed theoretically and experimentally that the ratio of mobility to diffusion in a gas is equal to the ratio of the charge per unit volume to the gas pressure. This is equivalent to saying that the Faraday constant—the electric charge per mole—is equal to the Avogadro number times the charge of an ion. In this way he proved that the charge of a gaseous ion and that of a monovalent ion of an electrolyte are the same.

Townsend also studied properties of the corona discharge which develops between a wire and a coaxial cylinder and he investigated discharges from points. He developed a theory for the cylindrical case and obtained a relation between the current per unit axial length as a function of the applied voltage, the mobility of the ions and the geometry, which describes the observations in many respects.

(*c*) *Activities during the first world war*

Little information is available about Townsend's movements during the first world war though it is known that he rendered most distinguished services to wireless telegraphy in the Navy. It has been reported that in 1914 he volunteered for a wireless unit which was to be sent to Russia to assist the Russian Army in training their cadets.

Townsend's mission was regarded by his colleagues in the Electrical Laboratory as a dangerous enterprise. One of them is said to have called at his home shortly before Townsend's departure, found him absent and left a message asking that the Professor should not forget to write a note to the University authorities recommending the caller as his successor to the Wykeham Chair if the Professor lost his life on the journey.

All that is known about this mission to Russia is that the plan fell through. However, Townsend took up wireless research for the Royal Naval Air Service with the rank of a Major on the special list (R.N.V.R.). He worked for several years at Woolwich where he developed, for example, small robustly constructed and accurately calibrated wavemeters. While he was in the Services, researches at his laboratory in Oxford were discontinued. The Electrical Laboratory together with other Museum buildings was taken over by the Royal Flying Corps for the training of cadets. However, he visited Oxford during the war on many occasions, as can be seen from the existing correspondence he had with the University authorities. His first book, *Electricity in gases*, which he wrote before the war, appeared in 1915.

It is sometimes said that the first world war had an adverse effect in later years on Townsend's scientific productivity and pre-eminence. In Lord Cherwell's appreciation of Townsend it is stated that the immense labour of building his new laboratory and the first world war proved a grievous interruption of Townsend's meteoric career. This seems to suggest that after 1918 his days of great discoveries were over. This conclusion would be fallacious for it was after the first world war that Townsend discovered a new physical effect which later on became of the greatest importance in the understanding of the wave-like nature of the electron. The fact that about the same time a German physicist discovered and analyzed this effect by more elegant means and with greater precision does not diminish Townsend's achievement. This has been acknowledged by modern writers in this field by calling it the Ramsauer–Townsend effect—a term Townsend would probably have resented.

3. Townsend's researches between 1919 and 1941

During the first world war, high-vacuum valves were applied for the first time as detectors, amplifiers and oscillators for wireless communication and other special purposes. Townsend used these valves extensively and carefully studied their properties. This can be seen from one of the first papers he published after the war, in which he dealt with the problem of frequency jumping or 'ziehen'. This troublesome effect was found very early with triode-valve oscillators of the self-excited type. When the valve circuit is strongly coupled with the resonance circuit the current near the resonance point changes discontinuously when the inductance is varied continuously; the sudden change is accompanied by a change in wavelength and the jumping has a hysteresis. This problem Townsend solved by a relatively simple mathematical analysis.

Subsequently he devised a novel method of measuring a wavelength up to about two orders of magnitudes larger than that of a standard short-wave generator which consisted of a Lecher wire system and a triode valve. The principle applied was to observe the beats with a telephone when the short-wave system interacted with the harmonics of the long-wave circuit.

In spite of his interest in high-frequency work, which has an important bearing on his later researches, Townsend took up again investigations on electrical phenomena in gases. From his published work it appears that even at that time his fundamental ideas about ionization of gases by electrons had not been accepted by his fellow scientists. In one of his papers on the collision theory he calculates the average energy to produce an ion pair in air when an electron moves through a gas in a moderate electric field and finds values around 25 V. He concluded that this value should not be confused with any critical potential, which should be smaller. He added that the 'principal error in the calculation is probably due to taking Maxwell's law as giving the distribution of velocities of electrons acted on by an electric force'. He also recognized clearly that a relatively small number of collisions of electrons lead to ionization.

When studying the motion of electrons in argon (which he freed from the major impurities) Townsend found that the mean free path of electrons in argon for mean electron energies below 1 eV was more than 50 times their value in hydrogen. He carried out observations in argon + hydrogen mixtures from which he calculated the dependence of the mean free path of electrons in argon on the mean electron energy and he obtained a curve which showed the well-known maximum. The unusually large transparency of argon for electrons of low energy is stressed in Townsend's paper; he remarks that the mean free path obtained from measurements with electron swarms is only a value averaged over the electron energies and that the actual mean free path for electrons of a given low energy must be much greater than the value measured by him. However, Townsend never pursued this research with mono-energetic electrons.

Townsend was well aware of the fact that only a knowledge of the actual distribution of electron energies would give him the answer to the question about the ionization rate by an electron swarm. It is for this reason that he returned to this problem again and again. In 1930, for example, he derived from his distribution law the number of electrons exceeding a certain energy and I find it significant that the figures he quotes lie between the Maxwellian and the Druyvesteyn distribution. In a later paper he points out that the variation of the mean free path of the electrons with energy has a considerable effect on the distribution of energies and he shows how in certain cases it can be allowed for.

The number of fast electrons in a distribution can be derived independently from optical or chemical effects associated with the motion of electrons. Townsend and his pupils started, therefore, to investigate the visible spectra emitted from electrically excited monatomic gases. He observed the light emitted from the positive column of discharges by means of selenium cells and colour filters, and measured the various parameters when the same light intensity was obtained through a certain filter. Assuming his distribution law he concluded that in helium there are too few fast electrons to account for the light and hence his findings were not consistent with Bohr's model of the helium atom. He was well acquainted with modern ideas of collision processes and he discussed in the same paper the possible effects of two-stage processes and collisions of the second kind.

Further work on helium at medium pressures showed that a positive column which was excited by an electrodeless discharge emitted band-spectra characteristic of molecular helium. In the discussion of the results he also considered whether the light could be produced by slow electrons hitting metastable atoms—which shows that Townsend did not exclude the possible existence of metastable states.

The influence of a magnetic field on the motion of charges in ionized gases presented another intricate problem which Townsend and his collaborators treated with considerable success. Under certain conditions of experiment a magnetic field is in many respects equivalent to an increase in gas pressure. Townsend developed the theory of diffusion of electron swarms in a magnetic field in the absence of space charges in which he describes analytically their angular motion about a given axis, a theory which culminates in the well-known result that the diffusion coefficient for the lateral motion of electrons decreases as the magnetic field is increased. Experimental work has confirmed the correctness of these ideas.

During his last years in office Townsend was much occupied with two further problems. One was the mechanism of the starting of electrodeless discharges in high-frequency electric fields, including the effect of a steady magnetic field perpendicular to the electric one. He developed a theory from which the variation of the starting field with the gas pressure and the frequency could be deduced and he made some experiments in support of it.

The other problem was that of the role of positive ions in feebly ionized gases. Townsend thought originally that positive ions do not take part directly or indirectly in the ionization process. When he studied multiplication processes in greater detail he concluded that positive ions may either release secondary electrons from the cathode or ionize gas molecules by collisions. Townsend always hoped to be able to devise a crucial experiment by means of which he would be able to discriminate between these two (and perhaps other) secondary effects. He and his pupils carried out a series of ingenious experiments but the numerical results obtained were not entirely convincing. However, it must be said in all fairness that this last problem has still not found a completely satisfactory answer.

4. TOWNSEND'S WORK DURING THE YEARS OF RETIREMENT

In 1941 Townsend retired from the Wykeham Chair. Feeling still young and vigorous at 73, he went to Winchester where he taught for a short time. Later he returned to Oxford to write a monograph on *Electrons in gases*. In this book he summarizes the work which he and his pupils have carried out since he wrote the book *Electricity in gases* which was published in 1915. In it he discusses the various objections which have been made against his theory of ionization by collision. In the final chapter he comes to the conclusion that experimental evidence on discharges from positive wires can only be interpreted in the way that positive ions contribute to gas ionization by direct collisions with molecules.

Two years afterwards Townsend wrote another smaller book, on radio communication, in which he explains by elementary mathematics the properties of electro-magnetic waves at large distances from their sources, dispensing with complex variables and avoiding Gauss's theorem and refined calculus.

One of Townsend's hobby-horses was always to pick holes in Maxwell's electro-magnetic theory and it was the most vulnerable part which he attacked, namely the assumption of a displacement current. In his last book, *Electromagnetic waves*, Townsend said that he found it difficult to explain the electrostatic forces which are produced by an oscillating charge on a sphere and he developed for this reason another propagation mechanism, which he hoped would be carefully considered and perhaps precipitate a vigorous controversy. The gist of his new theory was that electro-magnetic waves are propagated by a kind of ether which resembles a gaseous medium consisting of uncharged particles of mass smaller than that of electrons which have random velocities greater than the velocity of light. This ether, which is assumed to penetrate the space between the atoms, is the active component in the transfer of momentum from the electrons in the metal into the outer space.

During his last years he was writing a book on sound which was entirely based on the classical kinetic theory of gases in which he wanted to follow up certain ideas which he had indicated in various chapters in *Electromagnetic waves*.

5. Townsend as a human being

When Townsend arrived in Oxford in 1900 he was given rooms at New College where he lived for many years. In 1911 he married Mary Georgiana, daughter of P. F. Lambert, of Castle Ellen, County Galway, and they lived in a house within the precincts of New College. After the first world war he moved with his wife and his two sons to a house in Banbury Road, less than 10 minutes' walk from his laboratory. On his way he passed every day the Engineering Laboratory which was founded with his help, although at one time his plan met with considerable opposition from University circles.

He was very fond of walking through the University Parks, where he was once confronted with a most unexpected problem. There on the grass he met a man who had just broken his wooden leg by stepping into a hole normally reserved for a goal post. Townsend hurried back to his laboratory, persuaded his mechanic to fashion immediately a new leg and presented it to the unfortunate man who said that it was the best leg he ever had.

In his early Oxford days Townsend was a great huntsman and he was often seen riding on his horse to the laboratory. He was good at tennis, a game which he played until he was well over 70 years of age. He was known to enjoy arguments. It is said that when he and another professor examined a pupil for an advanced degree he took strong exception to the form of a question put by the external examiner to the candidate. The rest of the viva consisted of an argument between the two examiners.

He was a charming conversationalist and his stories about people he had met and other anecdotes of an amusing nature were always well received in common-rooms and at private parties. He disliked publicity, held very firm political views, spoke rarely in Congregation and took little notice of formalities associated with University affairs. He was generous to his staff and most helpful to his research workers. He was an untiring worker who ruled his kingdom strictly and often expected those with him to work long hours. He was an excellent draughtsman with a thorough knowledge of mechanical constructions and materials, and his experimental skill was very remarkable.

In one of his last lectures in 1940 to honours students he showed with greatest ease experiments on refraction, reflexion and diffraction with a 10 cm magnetron valve oscillator and I recollect the great satisfaction it gave him to demonstrate these fundamental properties of electromagnetic waves with modern means.

I also remember the first time I met Townsend in his laboratory. He hardly touched upon the correspondence between us 10 years previously but plunged straight into an account of arguments he had had, particularly with some of his German colleagues and the difficulties he encountered in convincing physicists of the correctness of his new ideas. When war broke out he stopped research in his laboratory and concentrated on teaching cadets. One day he asked me whether I wished to continue experimental research and when I answered in the affirmative he said: 'Take all my apparatus and bottles with rare gases and use them for your work. I shall not need them any longer.' This was typical of a man whom I admired for his directness, clarity of mind and informality.

Townsend had a striking personality and induced his pupils to work and think along lines which he alone decided. Most of his papers under joint authorship were written by him as can be seen from the characteristic style. The papers were not always easily readable and this may be one of the reasons why his new ideas did not often find a fertile ground in the minds even of progressive students of this subject. In general discussion Townsend and his pupils tended to be so absorbed by their own researches that they were rather impatient in explaining unfamiliar concepts to other workers. For many years they held fort on their isolated island, some of them occasionally carrying the flag into a far land. Some of them have imbibed their master's spirit, surely a sign of his commanding character.

Only on very few occasions was Townsend seen to attend scientific meetings. In 1924 he went to the U.S.A. to attend the centenary celebrations of the Franklin Institute at Philadelphia where he gave an address on the motion of electrons in gases. When the first international conference on ionization phenomena was held in Oxford in 1953, most of the foreign delegates met for the first time the man to whom they owed so much and who had opened up a field of study which had grown in size and importance beyond the limits he ever expected.

Townsend received many honours; in 1903 he was elected a Fellow of the Royal Society, he was made a Chevalier de la Légion d'Honneur, a member of the Institute of France, a member of the Franklin Institute and received an Honorary D.Sc. of Paris. He was knighted in 1941.

Townsend died at Oxford on 16 February 1957 at the age of 88, being survived by his wife and his two sons. Until his last day he was the distinguished scientist, still active in his work. He enjoyed a happy family life. He was proud of his wife who was a stimulating companion, a delightful hostess and an active worker in social and municipal affairs of the City of Oxford of which she is an alderman.

I wish to acknowledge the help I have received in preparing this memoir from Lady Townsend, Sir Douglas Veale, C. H. Paterson, J. A. Cochrane, J. V. Luce, Professors J. Mitchell and H. H. Plaskett and Dr G. Francis.

A. von Engel

BIBLIOGRAPHY

(Publications of Townsend's school which he communicated, but of which he was not a joint author are not included here.)

(*a*) *Papers*

1896. Magnetization of liquids. *Proc. Roy. Soc.* **60**, 186.

1897. On electricity in gases and the formation of clouds in charged gases. *Proc. Camb. Phil. Soc.* **9**, 244.

1897. Electrical properties of newly prepared gases. *Proc. Camb. Phil. Soc.* **9**, 345.

1898. The formation of clouds with ozone. *Proc. Camb. Phil. Soc.* **10**, 52.

1898. Electrical properties of newly prepared gases. *Phil. Mag.* (5), **45**, 125.

1898. On secondary Röntgen rays. *Proc. Camb. Phil. Soc.* **10**, 217.

1898. Applications of diffusion to conducting gases. *Phil. Mag.* (5), **45**, 469.

1899. Diffusion of ions into gases. *Phil. Trans.* A, **193**, 129.

1899. Diffusion of ions into gases. *Proc. Roy. Soc.* **65**, 192.

1900. The diffusion of ions produced in air by the action of a radio-active substance, ultra-violet light and point discharges. *Phil. Trans.* A, **195**, 259.

1900. The conductivity produced in gases by the motion of negatively charged ions. *Nature, Lond.* **62**, 340.

1900. The diffusion of ions produced in air by the action of a radio-active substance, ultra-violet light and point discharges. *Proc. Roy. Soc.* **67**, 122.

1901. Conductivity produced in gases by the motion of negatively charged ions. *Phil. Mag.* (6), **1**, 198.

1901. (With P. J. Kirkby.) Conductivity produced in hydrogen and carbonic acid produced by the motion of negatively charged ions. *Phil. Mag.* (6), **1**, 630.

1901. Uber die Leitfähigkeil in Gasen, erzeugt durch die Beiregung negative geladener Zonen. *Phys. Z.* **2**, 483.
1902 The conductivity produced in gases by the aid of ultra-violet light. *Phil. Mag.* (6), **3**, 557.
1902. Identity of negative ions produced in various ways. *Nature, Lond.* **65**, 413.
1903. The conductivity produced in gases by the aid of ultra-violet light. *Phil. Mag.* (6), **5**, 389.
1903. Specific ionization produced by corpuscles of radium. *Phil. Mag.* (6), **5**, 698.
1903. Some effects produced by positive ions. *Electrician,* **50**, 971.
1903. General theory: the theory of ionization by collision. International Electrical Congress of St. Louis 1904 (advance copy) (Section A).
1904. (With H. E. Hurst.) The genesis of ions by the motion of positive ions, and the theory of the sparking potential. *Phil. Mag.* (6), **8**, 738.
1904. Note on the potential required to maintain a current in a gas. *Phil. Mag.* (6), **8**, 750.
1905. A theory of the variation of the potential required to maintain a current in a gas. *Phil. Mag.* (6), **9**. 289.
1906. The field of force in a discharge between parallel plates. *Phil. Mag.* (6), **11**, 729.
1908. The charges on positive and negative ions in gases. *Proc. Roy. Soc.* A, **80**, 207.
1908. The charges on ions in gases and the effect of water vapour on the motion of negative ions. *Proc. Roy. Soc.* A, **81**, 464.
1910. The charges on ions in gases and some effects that influence the motion of negative ions. *Proc. Roy. Soc.* A, **85**, 25.
1911. On the conductivity of a gas between parallel plate electrodes when the current approaches the maximum value. *Proc. Roy. Soc.* A, **86**, 72.
1911. Charges on ions in gases. *Phil. Mag.* (6), **22**, 204.
1911. The mode of conduction in gases. *Phil. Mag.* (6), **22**, 656.
1911. The mode of conduction in gases. *Phil. Mag.* (6), **22**, 816.
1912. Determination of the coefficient of the inter-diffusion of gases and the velocity of ions under an electric force, in terms of mean free paths. *Proc. Roy. Soc.* A, **86**, 197.
1912. Diffusion and mobility of ions in a magnetic field. *Proc. Roy. Soc.* A, **86**, 571.
1912. (With H. T. Tizard.) Effect of a magnetic force on the motion of negative ions in a gas. *Proc. Roy. Soc.* A, **87**, 357.
1912. The charges on ions. *Phil. Mag.* (6), **23**, 677.
1912. Theory of ionization by collision. *Phil. Mag.* (6), **23**, 856.
1913. A theory of the glow discharge from wires. *Electrician,* **71**, 348.
1913. (With H. T. Tizard.) The motion of electrons in gases. *Proc. Roy. Soc.* A, **88**, 336.
1913. Low potential discharges in high vacua. *Phil. Mag.* (6), **26**, 730.
1914. Energy required to ionize a molecule by collision. *Phil. Mag.* (6), **27**, 269.
1914. (With P. J. Edmunds.) The discharge of electricity from cylinders and points. *Phil. Mag.* (6), **27**, 789.
1914. The potential required to maintain currents between coaxial cylinders. *Phil. Mag.* (6), **28**, 83.
1920. The collisions of electrons with molecules of a gas. *Phil. Mag.* (6), **40**, 505.
1920. Oscillations obtained by coupling a secondary circuit with a continuous wave valve oscillator. *Radio Rev.* **1**, no. 8 (May).
1921. (With J. M. Morrell.) Electric oscillations in straight wires and solenoids. *Phil. Mag.* (6), **42**, 265.
1921. (With V. A. Bailey.) Motion of electrons in gases. *Phil. Mag.* (6), **42**, 873.
1922. Velocity of electrons in gases. *Phil. Mag.* (6), **44**, 384.
1922. (With V. A. Bailey.) Motion of electrons in argon and in hydrogen. *Phil. Mag.* (6), **44**, 1033.
1922. Ionizing potential of positive ions. *Phil. Mag.* (6), **44**, 1147.
1923. Ionization by collisions. *Phil. Mag.* (6), **45**, 444.
1923. Ionization by collisions in helium. *Phil. Mag.* (6), **45**, 1071.
1923. (With V. A. Bailey.) Motion of electrons in helium. *Phil. Mag.* (6), **46**, 657.
1924. (With S. P. McCallum.) Electrical properties of helium. *Phil. Mag.* (6), **47**, 737.

1924. (With T. L. R. Ayres.) Ionization by collision in helium. *Phil. Mag.* (6), **47**, 401.
1925. Motion of electrons in gases. *J. Frankl. Inst.* **200**, 563.
1925. Note to V. A. Bailey's paper on the attachment of electrons to gas molecules. *Phil. Mag.* (6), **50**, 825.
1926. (With C. M. Focken.) Transference of energy in collisions between electrons and molecules. *Phil. Mag.* (7), **2**, 474.
1928. (With S. P. McCallum.) Electrical properties of monatomic gases. *Phil. Mag.* (7), **5**, 695.
1928. (With R. H. Donaldson.) Electrodeless discharges. *Phil. Mag.* (7), **5**, 178.
1928. Theory of high frequency currents in gases. *C.R. Acad. Sci. Paris*, **186**, 55.
1928. Motion of electrons in gases. *Proc. Roy. Soc.* A, **120**, 511.
1928. (With S. P. McCallum.) Electrical properties of neon. *Phil. Mag.* (7), **6**, 857.
1929. (With S. P. McCallum.) Ionization by collision in monatomic gases. *Proc. Roy. Soc.* A, **124**, 533.
1929. (With W. Nethercot.) High frequency discharges in gases. *Phil. Mag.* (7), **7**, 600.
1930. Energies of electrons in gases. *Phil. Mag.* (7), **9**, 1145.
1931. (With F. Llewellyn Jones.) The excitations of the visible spectrum of helium I. *Phil. Mag.* (7), **11**, 679.
1931. (With F. Llewellyn Jones.) The excitation of the visible spectrum of helium II. *Phil. Mag.* (7), **12**, 815.
1931. Elastic collisions. *Proc. Roy. Soc.* A, **134**, 352.
1931. Uniform positive columns in electric discharges. *Phil. Mag.* (7), **11**, 1112.
1931. Mean free path of electrons. *Ann. Phys. Lpz.*, (8), **7**, 805.
1932. (With M. H. Pakkala.) Excitation of continuum and line spectra in helium. *Phil. Mag.* (7), **14**, 418.
1932. Electrodeless discharges. *Phil. Mag.* (7), **13**, 745.
1932. (With F. Llewellyn Jones.) Ionization by positive ions. *Nature, Lond.* **130**, 398.
1933. (With F. Llewellyn Jones.) Ionization by positive ions. *Phil. Mag.* (7), **15**, 282.
1933. (With E. W. Townsend.) Intensity of radiation from uniform columns in discharge tubes. *Phil. Mag.* (7), **16**, 313.
1933. Distribution of energies of electrons in gases. *Phil. Mag.* (7), **16**, 729.
1933. Uniform columns in electric discharges. *J. Roy. Coll. Sci.* **3**, 108.
1934. (With S. P. McCallum.) Ionization by collision in helium. *Phil. Mag.* (7), **17**, 678.
1934. (With G. D. Yarnold.) Ionization by positive ions in helium. *Phil. Mag.* (7), **18**, 594.
1935. Theories of ionization. *Phil. Mag.* (7), **20**, 242.
1936. Distribution of energies of electrons. *Phil. Mag.* (7), **22**, 145.
1937. Equations of motion of electrons in gases. *Phil. Mag.* (7), **23**, 481.
1937. Deflexion of a current in a gas by a magnetic force. *Phil. Mag.* (7), **23**, 880.
1937. A comment on the paper by Sten von Friesen, 'On the values of fundamental atomic constants'. *Proc. Roy. Soc.* A, **163**, 188.
1938. Diffusion of electrons in a magnetic field. *Phil. Mag.* (7), **25**, 459.
1938. (With E. W. B. Gill.) Generalization of the theory of electrical discharges. *Phil. Mag.* (7), **26**, 290.
1939. Ionization by collisions of positive ions. *Phil. Mag.* (7), **28**, 111.
1943. Obituary of J. B. Perrin. *Obit. Not. Roy. Soc.* **4**, 301.

(*b*) *Books*

1915. *Electricity in gases.* Oxford: Clarendon Press.
1920. *Die Ionization der Gase.* (See E. Marx, *Handb. Radiol.* 1, Leipzig: Akademische Verlagsgesellschaft; in German.)
1925. *Motion of electrons in gases.* Oxford: Clarendon Press.
1943. *Electricity and radio transmission.* Winchester: Warren & Son.
1947. *Electrons in gases.* London: Hutchinson.
1951. *Electromagnetic waves.* London: Hutchinson.

John W. Trevan

JOHN WILLIAM TREVAN

1887-1956

Career

John William Trevan was born on 23 July 1887. His father was John William Stoneman Trevan of Plymouth and his grandfather was another John William Trevan. His father was originally a carpenter, but he was a very intelligent man and in these days he would perhaps have won a state scholarship. As it was he became clerk of works to a large building project and eventually went into business on his own account as a contractor in Plymouth. His wife Bessie Babbage was a simple country woman, but her son always treasured the theory, unsubstantiated by any proof, that he was descended from Charles Babbage, F.R.S., the inventor of calculating machines and one of the founders of the British Association.

The subject of this memoir was born in Bodmin, Cornwall, where his father was helping to construct the railway from Bodmin to Bodmin Road. He was the eldest of three children and had a brother, Jim, now in Canada, and a sister Winifred who has been dead many years.

His parents belonged to the Plymouth Brethren and he was brought up in the rigid practice of their faith. He was not allowed to read anything but religious books on Sundays or to play card games, and he was expected to believe that every word in the Bible was literally true. He was always religious, but in later life he was also broadminded and tolerant.

He went to the Regent Street School in Plymouth where, in spite of his undistinguished career as a diffident boy in a very large class, he was spotted by a master as having good potentialities. On the day of the scholarship examination for the Plymouth Scientific and Technical School (now the Plymouth and Devonport Technical College), when Trevan was fifteen, this master asked him why he was not a candidate. He replied that he had not asked his father, and the master said ‘Sit the exam. first and get your father’s permission afterwards’. By these words that master did a great service to science.

He attended the Technical School for three years and had the good fortune to be involved in an educational experiment. The school started a serious course of instruction in science when such courses were rare, and among the first fifteen boys to attend, a number besides Trevan became famous, including Professor Herbert Dingle the philosopher, Professor G. Hadfield the pathologist, Sir Lionel Horwill the judge, Mr Norman Lake who was senior surgeon at Charing Cross Hospital and Dr C. L. Williams who became Medical Officer of Health for Barking. This experiment, like other such experiments, was eventually too successful and the classes became overcrowded, but for a few years the atmosphere must have been most stimulating. Chemistry was taught to a high standard by Dr J. W. T. Thackrah, who was quiet and impressive with many little mannerisms which amused the students. Biology was taught by a stimulating man called J. Hodgson who had been biologist to Scott's first expedition in the Antarctic. The students spent their half-holidays collecting plants for him on Dartmoor and specimens from the seashore. Many of them matriculated in London; Trevan and Lake took the preliminary medical examinations in chemistry, physics and biology in 1906, and won scholarships at London hospitals.

Trevan had a brilliant career as a medical student at St Bartholomew's Hospital. He won the junior and senior scholarships, the Harvey prize in practical physiology, the Foster prize in anatomy, the Brackenbury Scholarship in medicine, and the Walsham and Burrows prizes in pathology. He obtained the B.Sc. with honours in physiology in 1908 and the medical qualifications of M.B., B.S. and M.R.C.S., L.R.C.P. in 1911.

He learned clinical medicine as clerk to Sir Wilmot Herringham, who was well before his time in the use of scientific methods of diagnosis. Neither he nor Drysdale, his assistant physician, was in consulting practice and they had plenty of time to develop the laboratory side of medicine. T. S. Hele, G. Graham, A. J. Clark, P. Hamill, J. W. Trevan, E. D. Adrian, R. A. Peters, G. Hadfield, I. de B. Daly, E. B. Verney and many other famous men were all connected with this same firm at different times.

Trevan became house physician to Herringham in 1911. He was never really interested in clinical medicine and was apt to regard each patient as a problem which was not only unsolved but insoluble, but when his colleague Dr P. Hamill went to the war in 1916 he entrusted the care of his father's fibrillating heart to Trevan with complete confidence. Trevan became M.R.C.P. in 1913, but then he plunged into laboratory work because he 'found clinical medicine too difficult'.

From 1912 to 1914 he held a Fishmongers' Company Research Scholarship and did experiments on gastric test meals in which contents of the stomach were ashed in three very valuable platinum bowls. These were left in a safe and Trevan, who was always absent-minded, developed the habit of losing the key.

From 1914 to 1920 he was demonstrator in physiology and casualty physician. The physiological department was run by J. S. Edkins, who was also lecturer at Bedford College, but Trevan was more impressed by F. A. Bainbridge (later F.R.S.), who was lecturer in pharmacology. Langdon Brown, who became senior physician at Bart's and Regius professor of physic at Cambridge, was also in the department and must have been a stimulating colleague.

Trevan thought deeply and was fond of drawing curves on graph-paper and skilled at making apparatus. For example, he used the lead screw of a lathe to drive the piston of a syringe in order to give a constant infusion of digitalis to a cat; a refined form of this apparatus became known later as the micrometer syringe. His first scientific publications were brief accounts to the Physiological Society of work on the kidneys and respiration carried out on behalf of the Medical Research Committee.

During this time Trevan was teaching physiology and he ran the whole department alone for about 12 months at one stage. His lectures, which were carefully prepared, appealed mainly to the clever students, and one of these, F. R. Winton, discovered that they were based on *The principles of general physiology*, by W. M. Bayliss.

In 1917 he joined the army as a Captain and pathologist in the R.A.M.C. and spent an entertaining 18 months in Cairo and Salonika. He was called home, with many others in Christmas 1918 because of the influenza epidemic in Great Britain.

In 1920 Trevan was appointed pharmacologist to the Wellcome Physiological Research Laboratories, which had already become famous through the work of H. H. Dale, G. Barger, P. P. Laidlaw and others. Most of his best work was done there in rooms on the ground floor looking out over the grass at Langley Court, Beckenham. My own first experience of research was under his guidance when he was most active (1925-26). He taught me to do things for myself, and this included making up standard solutions of acid and alkali, and helping in the construction of a static electrometer for measuring pH with a glass electrode. One day when he had lost something and suspected that it had been tidied away by his secretary, he went round muttering 'It's all due to this confounded tidiness. It always leads to trouble. I won't have it in my lab.'

Important early work with sulphonamides and the first experiments with diaminodiphenylsulphone were done by G. A. H. Buttle under Trevan's guidance. In 1941 he succeeded R. A. O'Brien as director of the laboratories and became a member of the Board of the Wellcome Foundation. A. C. White became head of the pharmacological department which inevitably grew as many new types of drug made their appearance.

The laboratories played an active part in the 1939-45 war. They were asked to prepare one million doses of tetanus antitoxin for the forces, and decided to make three million, which was fortunate because the final output reached three and a half million doses.

The manufacture of penicillin presented great difficulties; there was no room for it and no men could be spared for building. Eventually pressure was applied and a number of builders appeared, but it transpired that they had been obtained by releasing patients from a mental hospital!

In 1952 Trevan became Research Director of the Wellcome Foundation, succeeding C. H. Kellaway, and in 1953 he retired from this post and from the board, and was appointed consultant.

He was one of the original 29 members of the British Pharmacological Society when it was founded in 1931 and he became an honorary lecturer in pharmacology at University College London and a member of the Board of Studies in pharmacology in London University. When the Biometric Society was founded he became the first Vice-President of the British region. This international society is devoted to a branch of knowledge which owes much to Trevan and it was entirely appropriate that he should be at the head of the British section of it.

He played an active part in the work of the Research Defence Society; he served on the council for many years and was Chairman at the time of his death. 'To difficult and often delicate negotiations, he brought a great scientific reputation, complete personal integrity, and a serenity of outlook which disguised a firmness of will. Sure in his facts, he was a skilful and successful negotiator. Always fair and respecting the view of his opponent he knew when to call a halt.' (*Conquest*, **45**, 12, 1957).

He became a governor of St Bartholomew's Hospital Medical School (1937), Fellow of the Royal College of Physicians (1942) and President of the Section of Therapeutics and Pharmacology of the Royal Society of Medicine (1943). He served on the Biological Standards Commission of the League of Nations, on various committees of the British Pharmacopoeia Commission connected with biological standardization and on committees of the Medical Research Council and the Agricultural Research Council.

He was elected F.R.S. in 1946 and became Chairman of Government Grant board H.

He died suddenly and unexpectedly of coronary occlusion while he was working in his garden on 13 October 1956, at the age of 69. He had just promised to be press editor of the *British Journal of Pharmacology*. This would have involved much work and he must therefore have been feeling full of energy.

He left a widow and eight children, two of whom qualified in medicine, after following their father as students at Bart's.

Home life

When he was a boy he was fond of messing about in boats in Plymouth harbour and on the river and going for long walks and bicycle rides with his brother and school friends over Dartmoor. He often used to spend part of his holidays driving round Devonshire in a pony trap with a family of girl cousins from Paignton, who thought him dreamy and quiet; in later years these adjectives would not have been appropriate. He had an abiding affection for the West Country and he was very fond of talking about it. He used to boast that he had played bowls on Plymouth Hoe.

When he first came to London in 1906 he lodged with Mr and Mrs Percy Keys in Camberwell. They were zealous Baptists and it was thought that they would safeguard him from the evils of the metropolis. However, it was through them that he met and became engaged to Ida Kathleen Keys, the youngest daughter of the Rev. J. L. Keys, who was a Baptist minister and a cousin of Percy Keys. His parents opposed the match on religious grounds and the engagement lasted seven years. They were married in June 1916 after his father's death, and he was excommunicated by the Plymouth Brethren for marrying outside the sect. He was very sorry about this, but he did not let it rankle, and it did not dim his religious faith.

He lived for many years with his mother-in-law and her sons and daughters in a house in Grange Road, Upper Norwood, and his 3 sons and 2 daughters by his first wife were born there. When old Mrs Keys died he brought his own mother to live with them, too. Later he bought the house next door and lived there with his family until it was destroyed by bombs in October 1940. His wife became ill with cancer in the summer of 1937 and died at the end of that year and his sister-in-law came to live with them and looked after the children.

In September 1939 Trevan married Margaret, eldest daughter of Sir Hubert and Lady Llewellyn Smith, who had worked under him at Beckenham years before. They were married about an hour after the outbreak of war and spent most of their wedding night in an air-raid shelter. He was slightly wounded in the arm when his house was destroyed by bombs in 1940 but no one else was hurt. The family then moved to another house in Upper Norwood where they lived for 12 years. There were 2 daughters and a son by the second marriage and they moved in 1953 to a beautiful old rambling house with low ceilings and heavy beams in Woodside Green near Croydon. He was very fond of this place and became an enthusiastic gardener.

Trevan was a man of moods. Sometimes he was depressed and gloomy, but more often he was full of boyish enthusiasm. When the laboratories acquired a motor mower he tried it out himself and was seen seated upon it driving round the cricket field with a grin of beaming satisfaction on his face. He had an enormous zest for life and preferred to run rather than walk; to his death he always ran upstairs. He was interested in people, their schemes, ideas, and problems, and always ready to help them. He was fascinated by wild birds and flowers and loved trying to identify them. He was an omnivorous reader and remembered most of what he read. He said of himself, 'My relaxations have been at various times playing the piano, singing in church choirs, badminton, lawn tennis, cricket, hockey, chess and sailing, all performed with wild enthusiasm and devastating incompetence. But my experience leads me to believe that "rabbits" get more fun than the experts. The only hobby at which I have been any good has been the acquisition of miscellaneous information about anything in which a man can be interested.'

He loved motor cycling and after the first world war he rode a highly dangerous machine—an A.B.C. with a Sopwith engine, which had a spectacular acceleration up to 80 m.p.h. He used to wear a bowler hat for motor cycling, partly to protect his head in case of accidents, and partly because the police would be unlikely to think that a man wearing a bowler hat could be exceeding the speed limit. He once had a specially interesting skid on the ice in Croxted Road when he and the bicycle, both rotating on their axes, rotated about one another on the road and, he said, prompted some interesting reflexions on the dynamics of the origin of the solar system, even before he came to rest. Later he became devoted to second-hand cars to which he used to give first-aid with copper wire and other effective but unconventional materials. He was fond of touring on the continent, and once in early Nazi days he was in Rothenberg with a car and had finished lunch when a large German came up and said without any preamble, 'Sir, you have the best type of English face—honest and intelligent'. It was an odd compliment, but it pleased Trevan.

He was very youthful in spirit and had great sympathy for young people and their doings: at one time he was a Sunday school teacher. Before he had a family of his own he would spend his spare time taking young people to Maskelyne and Devant's or to concerts or museums or to the Royal Institution. Later he was deeply attached to his own family, as they were to him, and he gave them fully of his love, sympathy, help and encouragement. He loved parties, especially if they involved singing or dressing up for charades. He invented a variant of badminton for the children to play in the garden and called it 'goodminton'. He never minded making himself ridiculous and was fond of telling tales against himself.

He was gloriously absent-minded. During the war he used to bring home horse meat for the dog, but often the meat would stay in the back of the car all over a hot weekend to be carried back to Langley Court with its odour on Monday morning. After a bit his secretary used to tell Mrs Trevan by phone when horse meat was coming.

Once when he had been attending a meeting of the Board of the Wellcome Foundation he came out down a corridor whistling. The porter, not recognizing him, said 'Young man, please do not whistle so loudly. The directors are holding a board meeting.' 'That's all right', he said, 'we've finished our meeting.'

There was once a clergyman who went to visit the Bethlehem Mental Hospital in Park Langley and turned up in the Wellcome Laboratories by mistake. Dr Trevan came out of his room running and singing 'Rock of Ages'. The clergyman asked the receptionist 'How long has he been here?' and did not seem surprised when he was told 'A very long time'.

He enjoyed fun and games to the full, but he had the faculty of suddenly and completely detaching himself from his surroundings if an idea occurred to him. Out would come pencil and paper and usually a slide rule, or he would pick up a book and forget his surroundings for a time, and then quite suddenly he would look up, push his glasses on to his forehead and with a charming apologetic grin he would be back in the world again. He liked using graphical methods, but was prepared for more exact calculations when necessary. He once said, 'I do wish people would not use groups of six mice. They involve me in pages and pages of recurring decimals.'

He made thoughtful contributions to scientific discussions in a humorous and informal way which tended to conceal their importance. He was so ready to help others that he did not have enough time for his own work. His desk was always full of the results of experiments which he had performed with care but which he did not consider worth publishing. At the same time he devoted much effort to advising his younger colleagues about their experiments and about their publications. He had an encyclopaedic knowledge of medical science and his advice was always worth having, but he did not hesitate to express himself emphatically to make his points. 'My dear ass', he would say, 'You've got this all wrong', and the subsequent discussion was all great fun.

He was very fond of music. After a few hours' work as a student he would start playing extracts from Wagner's operas arranged for the piano. He was a determined rather than an expert performer and when he had mastered the Wagnerian chords he struck them in no uncertain manner. Later on he took delight in accompanying his family when they sang or played the oboe, 'cello or violin. He especially enjoyed playing duets on the piano and he would sometimes look up with an air of triumphant surprise when he finished at the same moment as his fellow performer.

He used often to slip into the church for an hour's playing on the church organ, of which he thoroughly understood the workings and to which he would render first-aid if some small thing went wrong. He also sang bass in the choir.

He dreaded responsibility, but never ran away from it. He was no respecter of persons and would stand up to the most eminent authority if he thought he was right and the authority wrong. He always stood for scientific and ethical truth against commercial expediency.

He was a very religious man. When he first came to London he continued to attend meetings of the Plymouth Brethren until his marriage. He then attended the local Baptist church, but after 7 years transferred his allegiance to St Aubyns Congregational Church, Upper Norwood. At a service in this church on 18 October 1956, the Reverend Norman Castles, M.A., paid him a moving tribute from which the following words are taken:

'The name "Trevan" for many years has been an integral part of this church. At his death John Trevan was a Deacon, a member of the Choir and a regular worshipper Sunday morning and Sunday evening. He had served as a leader in the Sunday School, as a Sidesman and as a Fabric Steward. But his real service was just himself, his charming personality, his complete dedication to the church and his many-sided accomplishments. He used to set a shining example by sitting with his sons and daughters in the body of the church and he had the great joy of seeing the older members grow to manhood and womanhood maintaining the family religious tradition. . . .

'Here in this church he came among us as an equal, conscious with us of the need for self-examination for refreshment of spirit and the religious duty of thanksgiving to God for all his goodness. In the Choir Vestry, before service, he would often lead us in prayer by saying the General Thanksgiving.

Each time you were made to feel that every word of gratitude in that comprehensive and incomparable prayer was the outpouring of John Trevan's own heart and mind to God. . . .

'But no tribute to John Trevan would be complete which did not recall his youthful buoyancy and his exuberant spirit. He had a puckish smile and a keen sense of humour. He loved to tell stories about himself, about his absent-mindedness, his unconventionality, his wit capped by a greater wit. He was never so happy as when he was at a party in his home or at a social in the Church. He delighted to be a character in a play, to dress up for a Choir chorus or to join in a nonsensical game. His versatility was an unending pleasure.'

Work

While Trevan was working in the department of physiology at Bart's he became interested in the respiratory centre. He divided the brain stem at various levels in cats and came to the conclusion that respiration was not controlled by a single centre but by a chain of neurones (1916). Later, with Miss Boock at Beckenham (1922), he observed great gasps of inspiration when the section was just below the colliculi, and concluded that a primitive respiratory centre below this level was normally under the control of a higher centre just above it. This work was overshadowed by the more detailed work of Lumsden (1923).

W. H. Hurtley was collecting evidence that many of the symptoms of diabetic ketosis were due to the presence of acetoacetates in the blood. He and Trevan (1916) therefore carried out experiments in cats which showed that acetoacetates increased the ventilation. They observed similar effects with acetylacetone and with salicylates, and suggested that all these substances owed their action to the presence of the group C(OH):CH. The stimulant action of salicylates on the respiration is now well recognized, but these early experiments on it and the ingenious suggestion that it is related to the hyperventilation of diabetes have been forgotten. Trevan also observed that acids in the blood, and these other chemicals, acted on the higher respiratory centre, and lost their stimulant action when the brain stem was divided just below the colliculi.

In 1921 Trevan and Ellen Boock published an interesting study of the effect of paraldehyde on the respiration. They plotted the ventilation in decerebrate cats against the pH of the blood, which was altered by giving varying amounts of CO_2. In the absence of an anaesthetic apnoea occurred with hyperventilation at about pH $7 \cdot 7$, and the ventilation increased rapidly as the pH fell. Under the action of paraldehyde the respiratory centre became insensitive to changes of pH so that not only did a fall of pH have less stimulant effect, but also a rise of pH was less liable to cause apnoea; eventually the ventilation became independent of the pH and the results were represented by a horizontal line.

In 1918 Trevan published a paper on the viscosity of the blood. After discussing various sources of error and precautions against them, he described experiments which showed that when the corpuscular volume was constant the viscosity of whole blood was proportional to that of its plasma, and that the effect of changes in the corpuscular volume was governed by a theoretical formula proposed by Hatschek.

During the early years at Beckenham Trevan devised several ingenious pieces of apparatus—a frontal writing point for use with ink, a microbalance and the micrometer syringe. The microbalance (1926) is a cheap, simple and sturdy deflexion balance in which a horizontal steel wire is fixed at one end and depressed by weights at the other end. The depression is measured by adjusting a micrometer screw to touch the end of the wire and weights of 1 to 30 mg can be weighed in 30 s with an accuracy of about ±0·01 mg. This device has been found useful in pharmacological laboratories for weighing out drugs for an experiment. It has the same functions as a torsion balance, but is cheaper and can easily be adjusted to other ranges of weight by changing the wire.

In the micrometer syringe (1922, 1925) the piston of a small glass syringe is controlled by a micrometer screw and small volumes can be measured with a standard deviation of about 0·2 cubic millimetres, so that titrations can be made with an error of 0·2 per cent when the total volume used is only 0·1 ml. The same principle had been used by Kelvin and Lister, but Trevan did not know this until after he had designed a compact and convenient instrument which was manufactured by Burroughs Wellcome and Co, and has been very widely used all over the world. One of its first applications was a method for estimating calcium in blood serum (Trevan & Bainbridge 1926) by a modification of the method of Kramer & Tisdall using only 1 ml of serum, or even 0·1 ml if great accuracy was not essential.

At about this time Trevan and Boock studied various problems which arose in connexion with the routine work of the laboratories. Frogs which had become pale after exposure to light were more easily killed by digitalis than frogs kept in the dark. When frogs were made pale by the injection of adrenaline, or dark by the injection of posterior pituitary extracts, the effects of digitalis showed the same correlation with the colour of the skin. Hogben & Winton had suggested that the effect of light on the colour of the frogs' skin was actually due to the release of hormones from the frogs' adrenals and pituitaries. The new results were 'at least not in contradiction with this view'.

Glenny & Allen had shown that an epizootic among guinea-pigs could be controlled by changing the diet and Boock & Trevan investigated the factors involved. They showed that the original diet of grain and mangolds was deficient in vitamin A so that the guinea-pigs might suffer from keratomalacia, which was cured with cod-liver oil. For satisfactory growth it was also necessary to add calcium and caseinogen to the diet. The importance of vitamin D was not generally recognized at that time (1922) and its possible effects do not enter the discussion. The effects of caseinogen may perhaps have been due to vitamin B_{12}, but this was not known till many years later. The authors drew carefully worded conclusions and cannot have regretted what they said.

In 1927 Trevan & Boock made a quantitative study of the effect of pH on the activity of local anaesthetics. Gross had found that the effects of these drugs were increased by alkali and suggested that they were due to the free undissociated base. Regnier, on the other hand, had explained this phenomenon as an effect on the tissues. In the absence of quantitative data both explanations were valid, and it is now known that both types of effect may occur. Trevan & Boock instilled buffered solutions of various local anaesthetics continuously into the eyes of rabbits for 10 minutes and showed that this produced a change in the pH of the aqueous humour. They measured the threshold concentration and when they plotted its logarithm against pH they got a series of parallel straight lines for all the drugs tested except two. The line for conessine had double the slope of the other lines and this was explained by the fact that conessine is diacidic. The line for benzyl alcohol had no slope at all and benzyl alcohol does not dissociate. These facts confirmed the theory of Gross, but the slopes of the lines were somewhat less than that predicted by the simplest possible theory. Trevan & Boock discussed the reasons for this discrepancy and came to the conclusion that it was not due to a local anæsthetic action of the ions of the drugs, but to a difference between the final pH of the cornea and that of the applied buffer solution. The p*K*s of those drugs was determined with the micrometer syringe using a volume of about 0·03 ml.

Trevan & Buttle (1927) made a joint study of the actions of bacterial toxins in acute experiments. They tested the action of the toxin of *Vibrion septique* on rabbits uterus and other tissues suspended in salt solutions with their movements recorded on smoked drums. In concentrations similar to those occurring in the blood of a rabbit recovering from an average lethal dose, this toxin caused prolonged contractions of the uterus. The effect was reversible on washing and specifically antagonized by the appropriate serum. This fact could be used to titrate the toxin against the serum, and some quantitative data on the amounts of toxin neutralized by various concentrations of serum were obtained. *Bacillus welchii* toxin caused similar effects. When the uterus was exposed to small doses of either toxin it became insensitive to larger doses of both toxins. When rabbits were immunized with *V. septique* toxin their uteri were still sensitive to the toxin *in vitro*. The desensitization which was observed to occur *in vitro* was thus clearly quite a different phenomenon from the immunity which could be produced *in vivo*.

Trevan's most important contribution to knowledge was work which led to the use of statistical methods in pharmacology. His own informal account of how this started is as follows: 'In the early days Burroughs Wellcome insulin was tested not only at Beckenham but also in the National Institute for Medical Research. Some batches of insulin which I passed for potency were rejected by the Institute. This touched my pride (besides getting me into trouble with B.W. and Co). In actual fact neither the Institute nor Beckenham was right; neither of us had really done an adequate assay. So I got out an old *Encyclopœdia Britannica*, and spent several weekends "mugging up" statistics from Edgeworth's article on the subject. This eventually resulted in a publication which brought some sort of order into the existing chaos of biological assay, and which was the stimulus to a great deal of work by others which has elevated biological assay to the rank of a science.'

Many people were vaguely aware at that time that different animals of the same species did not always give exactly the same response to drugs, but it was tacitly assumed by most workers in this field that the differences between different animals of the same species were small and could be neglected. The toxicity of drugs was therefore determined by giving a series of different doses each to one animal, and recording the 'minimum lethal dose' actually observed in this way; it was generally believed that this dose would kill all 'normal' animals and that smaller doses would kill none. Discrepant results were explained on the theory that some of the animals were not 'normal'. Shackell had plotted the percentage mortality caused by drugs against the dose and obtained S-shaped curves, but the logical consequences of these observations were neglected, until Trevan proposed that toxicity should be defined in terms of the dose causing a given percentage mortality. He used the term LD50 (or, for example, LD90) to denote the dose killing 50 (or 90) per cent of the animals, and this convenient notation has been generally adopted all over the world.

Trevan collected a number of examples of dose-mortality curves and discussed their significance and interpretation in a famous paper published in 1927 in the *Proceedings of the Royal Society*. Shackell had pointed out that a dose-mortality curve could be regarded as the integral of the frequency distribution of the doses just necessary to kill each individual animal. Trevan calculated the distribution of these 'individual lethal doses' in various actual cases by graphical differentiation of experimental curves and found them positively skew, but did not at first realize that this skewness could be avoided by using a logarithmic scale of doses. He calculated the mean, mode and median of these distributions and after some discussion came to the conclusion that the best dose to measure was the median lethal dose—the LD50. Using the expression $\sqrt{(pqN)}$ for the standard deviation of the observed mortalities and various values of N (number of animals), he calculated in actual cases (*a*) the range of doses which would be expected (with a given probability) to cause a given mortality and (*b*) the range of results which would be obtained by injecting the LD50 and then estimating from the observed mortality by means of the curve what the dose had been. In the case of cocaine this method of calculation from the curve appeared to be justified by the fact that the shape and position of the curve seemed to be practically constant; the calculations then gave an estimate of the error. A general formula for this error was obtained by taking a straight line as an approximation to the dose-mortality curve.

In the cases of diphtheria toxin and insulin the position of the dose-mortality curve varied, but its shape remained approximately constant, in the sense that curves obtained on different occasions could be made to coincide by an appropriate choice of the scales of doses. In this sense the shape of the curve seemed to be characteristic of the drug and the other conditions of the experiment, and the curves were therefore called 'characteristic curves'. It was clear that in these cases accurate assays would involve a comparison of the unknown with a standard preparation. This could be done either by using several doses of each drug and plotting two curves, or by using one dose of each drug and converting the observed percentages into a dose ratio by means of a standard curve. The latter method was simpler and was found to give satisfactory results when used on a large scale for the assay of insulin by estimating the percentage of mice showing hypoglycaemic symptoms. This had then recently become an important commercial problem, and both for the sake of the patients and for the sake of those who bought and sold insulin accuracy was essential. This involved the use of unprecedented numbers of mice and there was a mild sensation when the figures were published. Reporters invaded the laboratory and failing to get satisfactory interviews with members of the staff, published accounts of imaginary interviews, with obviously inaccurate circumstantial details.

Assays using 27000 mice provided the data for calculations which showed that the use of a standard curve gave accurate assays and that the errors observed in practice agreed well with those calculated theoretically. In each assay 30 mice received the unknown and 30 mice received the standard preparation and these assays were repeated about 20 times on each of 12 batches of insulin. The standard deviation of a single assay was calculated directly as 12·65 per cent and indirectly from the shape of the curve as 12·45 per cent. The standard deviation for the mean result on each batch would therefore be less than 3 per cent. and the scientific world was astonished that such accurate assays were possible.

Methods of assay have been developed since that time which have the theoretical advantage that they do not depend on the assumption that the slope of the curve is constant, but Trevan showed that his method did actually give the results which his theory predicted. The success of these calculations was aided by the fact that large numbers of animals were used, and by the fact that, in most of the experiments, the result could be quite accurately predicted, so that the observed mortalities with the unknown and standard preparations must have been almost equal. This success was however a striking demonstration that pharmacological experiments could give results with an error which was not only reasonably small but also measurable. Trevan had not only shown that animals varied more than had been supposed; he made this variation the basis of accurate experiments.

These methods were also applicable in other fields, and during the next few years Trevan applied them to cardiac glycosides, immunology and chemotherapy. In assays of preparations of strophanthus and digitalis in collaboration with J. H. Burn and others he used characteristic curves which were assumed constant, but in 1928 he also used a method suggested by Krogh in which the mortality was plotted against log dose and the results fitted with straight lines. The use of a logarithmic scale was clearly convenient, since different preparations gave curves of similar shape at a constant horizontal distance from one another and this device has formed the basis of many later developments. It gave results agreeing well enough with the results calculated from characteristic curves.

In the same paper (1928) he briefly described two new methods of standardizing digitalis preparations by their action on isolated rabbit's auricle and by their effect when infused slowly into a vein in a frog with a micrometer syringe. Since digitalis contains a number of active principles in different proportions it was not surprising that the various different methods gave different results.

In 1929 Trevan wrote a paper applying similar methods to the assay of antidysentery sera. A standard dose of toxin was injected and the assay was based on the relation between the dose of serum and the percentage survival rate. A logarithmic scale of doses was used and a new formula was given for calculating the accuracy of the result. The flatness of the curve was calculated in terms of the quantity A which, in a later notation, is approximately equal to 35λ. A formula was also given for calculating the error of the estimate of A. In this kind of test, where the animals are used to estimate the excess toxin which has not been neutralized, the percentage error of the assay should be less when the doses of both toxin and antitoxin are increased. Trevan drew attention to this theoretical conclusion and quoted evidence that it was true.

Later in the same year he published a paper, based on the work of his colleague, H. J. Parrish, dealing with the assay of antipneumococcus serum by its effect on the survival rate of mice infected with living pneumococcus. When the survivors were counted at a standard time, similar methods were found to give satisfactory results, but the accuracy could be improved by basing the calculations on the relation between the dose and the survival time. In this case the number of animals for a given accuracy was approximately halved; later work has led to the theoretical conclusion that this is likely to be always approximately true, when quantal data are replaced by measured data. When the dose of the infecting culture (C) was increased the protection could still be obtained by increasing the dose of serum (S), and over a certain range these two quantities were connected by the formula $S=C^{0\cdot32}$. In these experiments, however, unlike those with dysentery toxin, the accuracy of the result did not increase very much when the doses were increased.

In 1931 Trevan applied similar principles to chemotherapy. W. H. Gray had been studying the chemistry of various antimony compounds used in the treatment of kala azar. With the collaboration of Miss H. W. Bainbridge and Miss A. P. Attwood, he tested the effects of some of these compounds in the treatment of mice infected with trypanosomes. If given as a single dose at the appropriate stage of the infection, these drugs caused the organism to disappear for a number of days from the blood and increased the survival time of the mice, but eventually the organisms reappeared and the mice died. Single doses of each were compared with single doses of stibamine glucoside, which was used as a standard, and the therapeutic effect was estimated from the survival time. If the survival times did not differ significantly, the chosen doses were considered equivalent, and if the survival times differed the experiment was repeated with fresh doses until the difference in effect was not significant. In this way it was possible to distinguish doses differing from one another by about 30 per cent, so that the accuracy was sufficient for practical purposes.

In order to measure the toxicity of these antimony compounds larger doses were given to uninfected mice and the relation between dose and mortality determined. It was found that if logarithms of doses were plotted against mortality most drugs gave curves indistinguishable from the integral of a normal frequency distribution. This important conclusion is now generally accepted as true and much of probit analysis is founded upon it. In this paper he also discussed the use of the term 'maximum tolerated dose' which was used to mean the maximum dose which had never been observed to cause death. He pointed out that this depended on the number of animals used and proposed a more precise definition.

A collaborative experiment on the toxicity of aconite (1932) showed that the susceptibility of different colonies of mice varied significantly and emphasized the necessity of basing all experiments on a direct comparison of the unknown with a standard preparation.

Trevan wrote two papers (1934, 1938) in collaboration with G. A. H. Buttle and T. A. Henry on the effect of cinchona alkaloids and allied substances in canaries infected with malaria (Roehl's test). The effects of the drugs were measured in terms of the delay in the onset of the infection, as measured by the appearance of parasites in the blood. A standard dose of quinine was always used and the ratio of the effect of the drug to that of quinine was calculated. This 'quinine ratio' did not of course measure the potency, but it arranged the drugs in order of potency which was all that was then necessary.

In 1939 Trevan wrote a paper on amphetamine, in which he described some experiments on analeptics by a technique which has become popular in more recent years. Mice were anæsthetized with paraldehyde and the duration of the anæsthesia was measured as the time until they began walking about, which was called the walking time. This might vary over a ten-fold range among mice receiving the same treatment and Trevan took the median time, in preference to the mean time, as a measure of the effect. This time was shortened by three types of drug, typified by amphetamine, ephedrine, and picrotoxin, the actions of which could be distinguished by observing the mice.

In 1946-1951 Trevan wrote several papers on the pharmacology of curare. With G. A. Mogey and P. A. Young he studied the assay of curare by its effect on the isolated diaphragm of a rat (Bülbring's method). The paralytic effect of curare on this preparation was found to be very constant and the dose-effect curve was very steep so that when sixteen responses were measured that part of the error of the final estimate of potency which was due to variations of the response was sometimes less than 1 per cent. In these circumstances the usual assumption that errors of measurement of the dose and of the volume of the bath are negligible may not be justifiable unless special care is taken.

Trevan gave a general account of his views on curare and allied drugs in the Bertram Louis Abrahams lecture to the Royal College of Physicians (1951).

He summed up his own work by saying that he had had the satisfaction of starting a number of hares most of which had finally been caught by others. It may perhaps be added that he was always willing to let others have the credit even when he was in at the death himself and, of course, some of the hares are still at large.

I am much indebted to many people who have helped me write this memoir, and especially to Mrs Trevan, Mr Ernest Freeman, Dr Graymore, Dr A. C. White and the writers of various obituary notices.

J. H. Gaddum

BIBLIOGRAPHY

1913 (With W. P. Herringham.) The localization of function in the kidney. *Quart. J. Med.* **6**, 505.

1914. The estimation of rennin in gastric contents. *Trans. Internat. Congr. Med. London IIIa Chem. Pathol.* p. 23.

1915. Dilatation of the heart. *St Bart's Hosp. J.*, **23**, 18.

1916. The excretion of acid by the kidney. *J. Physiol.* **50**, 15P.

1916. The control of respiration by the mid brain. *J. Physiol.* **50**, 43P.

1916. (With W. H. Hurtley.) The physiological action of acetoacetic acid and related substances. *J. Physiol.* **50**, 49P.

1917. (With F. A. Bainbridge.) Some actions of adrenalin upon the liver. *J. Physiol.* **51**, 460.

1918. The viscosity of blood. *Biochem. J.* **12**, 60.

1920 Some notes on relapsing fever. *St. Bart's Hosp. J.* **27**, 6.

1921. (With E. Boock.) The action of anaesthetics on the respiratory centre. *J. Physiol.* **54,** 131P.

1922. (With E. Boock.) The effect of section of the vagi on the respiration of the cat. *J. Physiol.* **56**, 331.

1922. An apparatus for the measurement of small quantities of fluid. *Lancet,* **i**, 786.

1922. A new frontal writing point for use with ink. *J. Physiol.* **57**, 17P.

1922. (With E. Boock.) The food value of mangolds and the effects of deficiency of vitamin A on guinea-pigs. *Biochem. J.* **16**, 780.

1923. (With E. Boock.) The effect of light on the response of frogs to drugs. *Proc. R. Soc. Med.* **16**, 8.

1923. (With H. W. Dudley and P. P. Laidlaw and E. M. Boock.) The effect of insulin on the respiratory exchange. *J. Physiol.* **57**, 47P.

1925. The micrometer syringe. *Biochem. J.* **19**, 1111.

1926. A modification of the deflection balance for use in biochemical laboratories. *Biochem. J.* **20**, 419.

1926. (With H. W. Bainbridge.) The estimation of calcium in blood serum. *Biochem. J.* **20**, 423.
1926. (With J. H. Burn.) The standard for the biological assay of strophanthus. *Pharm. J.* **117**, 439.
1926. (With E. Boock.) The standardization of insulin by the determination of the convulsive dose for mice. *League of Nations Health Organization Document.* Ch. 398, 47.
1927. (With E. Boock.) The relation of hydrogen ion concentration to the action of local anaesthetics. *Brit. J. Exp. Path.* **8**, 307.
1927. (With G. A. H. Buttle.) The action of *Vibrion septique* and *B. welchii* toxin on isolated organs. *Brit. J. Exp. Path.* **9**, 182.
1927. The error of determination of toxicity. *Proc. Roy. Soc.* B, **101**, 483.
1927. The medical aspects of the alkaloids. *Ann. appl. Biol.* **14**, 537.
1928. (With E. Boock, J. H. Burn and J. H. Gaddum.) The pharmacological assay of digitalis by different methods. *Quart. J. Pharm.* **1**, 6.
1929. A statistical note on the testing of anti-dysentery sera. *J. Path. Bact.* **32**, 127.
1929. (With W. Culhane, H. P. Marks and D. A. Scott.) Observations on insulin. Part 2. Physiological assay of crystalline insulin. *Biochem. J.* **23**, 384.
1930. The accuracy of titration of anti-pneumococcus serum. *J. Path. Bact.* **33**, 739.
1931. (With W. H. Gray.) Experiments on antimony compounds used in the treatment of bilharzia disease and kala-azar. *Trans. R. Soc. trop. Med. Hyg.* **25**, 147.
1931. (With W. H. Gray and H. W. Bainbridge.) The ureides of *p*-aminophenylstibinic acid. *Proc. Roy. Soc.* B, **108**, 54.
1931. The physiological action of some toxins. In *System of bacteriology in relation to medicine.* **5**, 194. Medical Research Council.
1932. (With W. A. Broom, J. H. Burn, J. H. Gaddum, and S. W. F. Underhill.) The variation in the susceptibility of different colonies of mice towards the toxic action of aconite. *Quart. J. Pharm.* **5**, 33.
1934. (With H. Paget and A. M. P. Attwood.) The irritant constituent of anti-leprotic oils. *Internat. J. Leprosy*, **2**, 149.
1934. (With G. A. H. Buttle and T. A. Henry.) The action of the cinchona and certain other alkaloids in bird malaria. II. *Biochem. J.* **28**, 426.
1936. Comparison of the proposed new international (crystalline) standard insulin with the current international standard. *League of Nations. Quart. Bull. Health Organization*, **5**, 622.
1938. (With G. A. H. Buttle, T. A. Henry, W. Solomon and E. M. Gibbs.) The action of the cinchona and certain other alkaloids in bird malaria. III. *Biochem. J.* **32**, 47.
1938. The introduction of new remedies into clinical practice. *Proc. R. Soc. Med.* **31**, 267.
1939. (With H. Paget and A. M. P. Attwood.) The irritant action of some drugs derived from hydnocarpus oil. *Int. J. Leprosy* **7**, 167.
1939. Benzedrine: uses and abuses. *Proc. R. Soc. Med.* **32**, 391.
1941. (With R. G. Macfarlane and A. M. P. Attwood.) Participation of a fat soluble substance in coagulation of the blood. *J. Physiol.* **99**, 7P.
1946. The international standard and unit for penicillin. *League of Nations. Bull. Health Organization*, **12**, 214.
1946. Curarine. *Lancet*, **ii**, 361.
1948. (With G. E. Glock and G. A. Mogey.) Pharmacology of some bis-tri methylammonium compounds. *Nature, Lond.* **162**, 113.
1948. (With G. A. Mogey.) Response of the phrenic nerve of the rat to rectangular pulses of direct current. *J. Physiol.* **107**, 28P.
1949. (With G. A. Mogey and P. A. Young.) The assay of d-tubocurarine chloride on the isolated rat diaphragm. A statistical examination. *Analyst*, **74**, 577.
1950. (With G. A. Mogey.) D-tubocurarine salts and derivatives. *Brit. med. J.* **ii**, 216.
1950. (With W. Neilson Jones.) Preservation of fungal sporophores. *Nature, Lond.* **165**, 692.
1951. Curare and curari-mimetic substances. Bertram Louis Abrahams lecture. *Sci. Progr. Twent. Cent.* **155**, 401.

Hubert M. Turnbull

HUBERT MAITLAND TURNBULL

1875-1955

HUBERT MAITLAND TURNBULL, who died on 29 September 1955 some eight years after retirement from the Chair of Morbid Anatomy at the London Hospital Medical College, occupied a position of eminence in British pathology. Not only was he greatly esteemed by his colleagues at the London but his influence extended widely throughout the medical schools of this and other countries of the Commonwealth. This was due not so much to his ability as an initiator and director of research, even though he was responsible for a considerable amount of valuable original work during his forty years at the London Hospital, but to a particular genius for accuracy of observation and meticulous attention to detail which he possessed in high degree and applied with almost religious fervour to everything that he did. Entering pathology at a time when many in this country held that morbid anatomy was a dead subject, Virchow, in their opinion, having left little new territory to be explored, Turnbull set himself to revolutionize morbid anatomical practice and to raise the subject to the level of a science. And so well did he succeed that he proved a source of inspiration not only to his fellow pathologists and those young graduates who chose to emulate him, but also to the much wider circle of clinicians who sought the privilege of working for a time in his department as a prelude to specialization in some other branch of medicine.

The son of Scottish middle class parents Turnbull was born on 3 March 1875 in Glasgow, but since the family moved to Edinburgh when he was still an infant it was this latter city that he came to look on as his native one. His father, Andrew Hugh Turnbull, was an actuary who at the time of his son Hubert's birth was Manager of the City of Glasgow Life Assurance Company, but later joined the Scottish Widows Fund, serving first of all as Secretary and Joint Actuary and subsequently as Manager. He was an enthusiastic member of the Volunteer Force and had the distinction of being elected in 1878 to the Queen's Body Guard for Scotland, the Royal Company of Archers. Turnbull's mother, Margaret Lothian Black, was the youngest daughter of Adam Black, the founder of the publishing firm of A. & C. Black which later moved from Edinburgh to London. Adam Black was twice Lord Provost of Edinburgh and represented that city in parliament from 1856 to 1865. In commemoration of his long and valued services a statue to him was unveiled in 1877; it stands today in Princes Street Gardens, Edinburgh. It is worthy of note that two of Turnbull's paternal uncles had distinguished careers. His father's eldest brother, John William Turnbull, who was at Caius College, Cambridge, and passed Senior Optime in the Mathematical Tripos coming fifth in the list, was later called to the Bar at Lincoln's Inn and subsequently went to Natal where he was eventually made a Puisne Judge of the Supreme Court of that country. Another uncle, Alexander Turnbull, studied medicine at Edinburgh University, taking his doctorate of medicine at the early age of 21. Entering the Navy he had a brilliant career culminating in his appointment as Inspector General of Medical Services. Thus on both sides of the family there is ample evidence of an ability which was above the average from which Turnbull could have inherited his particular qualities, if these things can be derived in this way.

He commenced his education at St Ninian's, a preparatory school in Moffat, where from 1884 he spent four years which seem to have been without any particular formative influences. If one were to search for such an influence in this period of his life it would probably be to mention William Evans the Scottish naturalist, a colleague of his father in the Scottish Widows Fund. This man befriended Turnbull and his young brother; he took them on bird-nesting expeditions and taught them how to observe the habits of birds and to recognize their song and alarm notes. This friendship had a very profound effect on Turnbull, in fact he attributes his interest in science to this association and he kept in touch with Evans right up to the time of his death. For the remainder of his schooling Turnbull came South of the Tweed to Charterhouse, where for six years he received the type of classical education which was provided by the English Public Schools in those days. The only concession to science teaching at Charterhouse at that time was to allot one hour a week to chemistry, and after reaching the Lower VIth even that meagre ration was dispensed with. Not only was there this great disparity in the amount of time devoted to the teaching of science and the classics, but the quality of the two showed an equal divergence; the teaching of the humanities was of a high order with T. E. Paige outstanding amongst the classical masters.

So when Turnbull left Charterhouse in 1894 to go to Magdalen College, Oxford, he had yet to receive some training in science, a fact which he deplored at the time, though in all probability it was not a serious handicap. And it was not until two years later, after taking Classical Moderations, that he turned to science with the object of qualifying in medicine. This change was largely the result of his father's suggestion, who no doubt had in mind his son's early interest in ornithology, and the wisdom of this advice was soon to become evident by the enthusiasm with which Turnbull threw himself into these new studies. Attracted in turn by botany, animal pathology and physiology it was this last subject in which he found the greatest interest. His teachers included Professor Gotch, Dr J. S. Haldane and Dr Gustav Mann and the excellence of their teaching combined with the small size of the classes provided unique opportunities for learning, of which Turnbull took full advantage. It was confidently expected that he would get a first in Greats but to the disappointment both of his teachers and himself he only obtained a good second class. It was about this time (1896) that an introductory course in pathology for medical students at Oxford was first instituted. This was done at the instigation of Professor Burdon-Sanderson, recently appointed to the Regius Chair of Medicine at Oxford, and the course, organized and given by Dr James Ritchie, was taken by those studying for medicine concurrently with anatomy after graduating B.A. In the summer of 1898 when Turnbull attended Ritchie's course there were six others in the class, amongst them A. E. Boycott and H. R. Dean, and it must be accounted in large part to Ritchie's teaching that three of the seven—Turnbull, Boycott and Dean—elected to specialize in pathology. All three came to occupy positions of eminence in this speciality. Anatomy in those days at Oxford was taught by Professor Arthur Thomson and Dr Gabriel Farmer. At first Turnbull found this a very difficult subject, but helped by a good visual memory he came to enjoy it, finding particular interest in its comparative aspect. And having taken the 2nd M.B. and been awarded the James Welch Prize in anatomy, he prepared for the primary F.R.C.S. examination by acting as demonstrator in anatomy at Oxford for a period of two terms, joining the extramural school of anatomy at Edinburgh in order to continue these studies during the vacations. The decision to specialize in pathology had not yet been taken, in fact this was not to be decided until after Turnbull had qualified and had studied with Professor Schmorl. At the time when he was about to embark on his clinical studies his aim, apparently, was surgery, at any rate this seems to have been the wish of his Oxford teachers, and he accomplished the first step in this direction in the autumn of 1900 by passing the primary F.R.C.S. examination.

It was at this time that he went to the London Hospital for his clinical studies, having obtained the Price University Entrance Scholarship which is open to competition at that medical school by graduates of Oxford and Cambridge. Amongst Turnbull's teachers at the London Hospital probably none made a greater impression on him than Henry Head and William Bulloch. Turnbull has described how when he first went to the London he used to meet Henry Head in the Underground on his daily journey to the hospital and how Head profited from these chance meetings to instruct him in the matter of physical signs. Though Turnbull counted this to his good fortune it was not without its embarrassment for his instructor's loud and penetrating voice caused considerable annoyance to their fellow passengers. This passed unnoticed by Head, however, who continued daily unperturbed to give these unsolicited lessons on the Underground. Later Turnbull came to be officially connected with Head when he served as one of his clinical clerks and this stimulating experience so raised his interest in neurological medicine that for a time neurology competed with surgery in Turnbull's plans for postgraduate specialization. It was doubtless also responsible for the fact that the thesis which Turnbull presented for the degree of D.M. was on a neuropathological subject: the apparently congenital loss of post-central cortex in an adult. Sherrington was invited to adjudge the thesis and he expressed the opinion that the candidate was a neurologist of great promise and since Professor Burdon-Sanderson, the Regius Professor of Medicine, was of a like mind, the thesis was accepted and Turnbull proceeded to the degree in 1906. William Bulloch, later to be made Professor of Bacteriology, was also a most inspiring teacher; that seems to have been the almost unanimous opinion of those who studied under him and it was undoubtedly shared by Turnbull. Not only was he widely read, but he knew personally many of the leading pathologists and microbiologists of Western Europe and he drew largely on this intimate knowledge to give his lectures a vivid interest which claimed the attention of even the least studious. Turnbull found a delight in these lectures which provided him with something that even the widest reading could not furnish, and it seems clear that the teaching of Bulloch, like that of Ritchie at Oxford before him, must have had something to do with Turnbull's election to make a career in pathology.

After qualifying at the end of 1902 by obtaining the Conjoint Diplomas of the English Colleges as well as the B.M. and B.Ch. of Oxford, Turnbull, following the usual practice, began what was intended to be a series of house appointments which had, however, to be curtailed on account of illness. Having served three months on Surgical Out Patients and acted for a short period as Emergency Officer, he was made House Physician to Drs Francis Warner and Bertrand Dawson (later to become Lord Dawson of Penn). Unfortunately this last post was held for only ten days when he fell ill and was eventually sent home, and it was not until some five months later that he returned to the London Hospital this time to work in the Institute of Pathology where Dr Redcliffe Salaman had recently been appointed to be the first director.

The idea, fostered by Turnbull's teacher at Oxford, that he should become a surgeon still dominated his plans for the future. And largely at the suggestion of Professor Ritchie, who seems to have played a prominent part in organizing Turnbull's postgraduate studies, it was suggested that he should go to Professor Schmorl at Dresden to learn something about bone pathology in preparation for specialization in orthopaedic surgery. With this in view Turnbull entered for the Radcliffe Travelling Fellowship of University College, Oxford, having been coached for the examination by Bulloch. The result of this examination has to be ratified by the Primate, the Lord Chancellor and the Premier, and Turnbull's departure for his studies abroad might have been considerably delayed had he been obliged to await the official announcement of his having been awarded the Fellowship. However, Professor Ritchie told him in strict secrecy of his success and in July 1904 he went to Jena for a few weeks to perfect his German, and returning to this country in August, he left again shortly afterwards, this time for Copenhagen, Here it was planned that he should attend Professor Salmonsen's course of experimental pathology and work with Dr Georges Dreyer (later to become Professor of Pathology at Oxford) at the Danish State Serum Institute, before going to Dresden where he was to spend the major part of his time. But again illness interfered with his plans. Hardly had he commenced his studies at Copenhagen than he fell ill and was admitted to the Kommune Hospital; he returned home after a stay of little more than two months in the Danish capital. And when by the spring of the next year his health had been restored Turnbull decided to proceed to Dresden without further delay to take up his duties as voluntary assistant to Professor Georg Schmorl at the Friedrichstadt Krankenhaus. This association with Schmorl was probably one of the most important episodes in Turnbull's life. Schmorl was of striking personality, a man of energy and great vitality. He had a profound knowledge of his subject and he was in addition an excellent teacher, Turnbull has described him as the most inspiring that he had ever met, and it did not take very long to persuade Turnbull that what he had been told in England about morbid anatomy being a dead subject was far from being true. As practised and taught by Schmorl it was very much alive, and before Turnbull's stay in Dresden was many months old he had decided to abandon all idea of a clinical career and to devote himself to morbid anatomy. When he had nearly completed a year with Schmorl news came from London that the directorship of the Institute of Pathology (then the Andrew Clark Institute) at the London Hospital was shortly to be advertised. Dr R. Salaman, its first director, had been obliged to retire on the score of ill health and Dr R. G. Slade who had succeeded him had died; it was temporarily under the directorship of Dr Charles Miller, who ultimately became the first director of the Medical Unit at the London Hospital. Since the directorship of the Andrew Clark Institute was the type of post which Turnbull now had in mind as his ultimate goal he returned to London to make further enquiries, and although on hearing of the duties of the post Turnbull thought that he was still somewhat inexperienced for it, he decided to apply and paid the necessary round of visits on the members of the consultant staff of the hospital. He was, however, frank with them about his lack of experience and most wisely Turnbull was told to return to Dresden and to re-apply when he felt that he was qualified to undertake these new duties, it being understood that Dr Charles Miller would continue temporarily to act as director until Turnbull was ready. After a further seven months with Schmorl he re-applied and was appointed to the post of Director of the Andrew Clark Institute of Pathology at the London Hospital in November 1906, a post that he was to hold until he retired forty years later.

The first few years of his directorship proved heavy going. Not that his duties as laid down by the hospital were onerous. He had to organize the Institute, see that the autopsies were satisfactorily made and recorded, conduct research and do some teaching of morbid anatomy and it is probable that Turnbull could, without undue effort, have fulfilled these requirements to the satisfaction of everyone but himself. He had other, more ambitious ideas, however. Morbid anatomy in England at that time was at a rather low ebb; its methods were perfunctory and it lacked the vital spark of a healthy growing subject. And Turnbull coming directly from the very invigorating experience of working in Schmorl's department at Dresden, had early decided to make radical changes in morbid anatomical practice at the London Hospital. He debated with himself whether to introduce these reforms one by one or to make a clean sweep; he decided on the latter alternative. This decision not only entailed a great deal of hard work but it inevitably brought Turnbull into conflict with those of his colleagues who were identified with the old regime and resented the change for one reason or another. And Turnbull was not perhaps the best person for this task in some respects. Not only was he imbued with all the zeal of the reformer but he could be particularly unyielding when he was convinced that right was on his side, and had it not been for the loyal help of his Assistant Director, Dr Robert Worthington, the difficulties might have proved insuperable. In addition to his professional qualities Worthington was a man of considerable tact and since he was also *persona grata* with his clinical colleagues he was able to further his chief's interests invaluably. Between the two the reforms were put through and in a relatively short time the Institute was running if not to the satisfaction of Turnbull, at least on what he considered to be the right lines. Much new equipment was purchased including a circular saw for the bones, the need for which had been impressed on Turnbull by Schmorl as a parting injunction when he left Dresden. A standard method of conducting a complete necropsy was laid down and books for recording the clinical and post-mortem findings and the examination of pieces of tissue taken for microscopy, were designed. The usual practice of recording size by comparison with natural objects—bean, pea, fruits, eggs, etc.—was replaced by accurate measurement and the need for a variety of staining methods in the microscopical examination of sections was emphasized. In this last respect Turnbull was embarking on the road which has led to the elaborate discipline of histochemistry of today. One of the major reforms instituted by Turnbull concerned the arrangements for the making of autopsies. Originally these had been done by the physicians of the hospital and when Turnbull took over the Institute four physicians attended one day each a week as 'visiting pathologists' to make and demonstrate necropsies. These gentlemen were far from punctual in their attendance and under the new regime this so disorganized the work of the department that complaints soon came from the post-mortem attendants owing to the amount of overtime they had to work. An attempt to fix the hours of attendance of the visiting pathologists was hotly resented by them and Turnbull was reported for censure first to the Medical Council of the hospital, who disclaimed any jurisdiction over him, and subsequently to the College Board, who found in Turnbull's favour. With the effective support of the Chairman of the Hospital, Sydney Holland (later to become Lord Knutsford), and the tactful intervention of the Assistant Director, Robert Worthington, peace was restored. But this really marked a turning point in the affairs of the 'visiting pathologists'. Gradually the number of autopsies made by them diminished; by 1911 few were so done and in another nine years this practice had ceased, all the necropsies being made by the Institute staff.

Another important change instituted by Turnbull concerned the examination of biopsy material. When he was appointed none of this work came to his department. Surgeons wishing to examine specimens removed at operation or at Out Patients had sections prepared for them by a technician working in the hospital. And when Dr Panton (later Sir Philip Panton) was appointed first director of the Clinical Laboratory in 1909 it was arranged that he should take over the work. This was much resented by Turnbull who argued with every reason that he could not possibly learn or teach his subject if he were to be confined to the study of material obtained at necropsy; under such an arrangement some types of tissue change would hardly ever come his way. So strongly did he feel on this matter that he intimated he would have to retire if it was not rectified. Fortunately a compromise was reached whereby the Institute was allowed to take over the examination of surgical biopsies, the surgeons being given the right to choose to which department—the Institute of Pathology or the Clinical Laboratory—they sent their specimens. And although when I went to the London Hospital in 1926 a few biopsy specimens were still being sent to the Clinical Laboratory, it was not long before the vast majority went to the Institute. The compromise was reached in March 1909 and by the end of that year nearly a thousand surgical specimens had been received; in 1946 the year of Turnbull's retirement, the number had risen to 3080. On these broad lines the work of the Institute developed over the succeeding years. The building in which it was housed was enlarged and the new building, renamed the Bernhard Baron Institute, was opened in 1927 by Sir Humphrey Rolleston. Under Turnbull's enlightened and devoted direction it became one of the leading centres of morbid anatomy in the country and a Mecca for those from all over the world interested in this subject.

Turnbull's value as a teacher is not easy to assess. He was probably not quite in the same class as some of our most eminent teachers of pathology, such as Hamilton of Aberdeen for instance, at any rate as a teacher of undergraduates he was not in this class. Yet his influence on pathology for which he will long be remembered, was very great. This was not due to any particular oratorical skill or gift for the inspiring presentation of his subject, for he was not a fluent lecturer. That at any rate is what I have gathered from others, because I never actually heard Turnbull lecture or even read a paper before a scientific society for that matter. He taught in the lecture room, of course, and from 1909, two years after his appointment, the Director's course of lectures constituted an important part of the teaching offered by his department. But though these lectures were in the first instance intended for the undergraduates and attended by them, as time went on this course was expanded into an advanced one, comprising lectures on both general and special pathology, more suitable for postgraduate than undergraduate consumption. And since from 1911 the undergraduates were specifically catered for in a course of lectures given by the Assistant Director, attendance by them at Turnbull's lectures ceased to be obligatory and his audience was drawn largely from the house officers, the pathology assistants and junior members of his staff. A few of the undergraduates from amongst the more serious and discerning continued to go to the Director's lectures, as I learned to my discomfiture when I became responsible for the teaching of bacteriology. Owing to an error on the part of the Dean's office I found myself billed to give one of my lectures at the same time as Turnbull and some half dozen of my class were torn between seeing how the new professor shaped and going to Turnbull's lecture. Despite representations to the Dean I obtained no relief from the somewhat unequal struggle for the rest of the academic year, in fact the Dean thought that it was rather a good arrangement that I should have to compete with a colleague for the attention of the students, it put me on my mettle right from the start! And as a matter of fact I did not come out of it too badly, though inevitably I lost one or two of the students to Turnbull. One thing it impressed on me, however, and it is for this reason really that I recount the episode, though Turnbull might not have been the most eloquent of lecturers the substance of his lectures was magnificent. They were complete in every detail, including all the most recent work; nothing went into those lectures unless it had been most carefully weighed and checked and authority was invariably given for views or statements about which there was question. They were, in fact, a veritable *tour de force* and vastly appreciated, but they were strong meat. And although a few of the undergraduates found their way into his audience the majority of the students at the London Hospital did not come under Turnbull's influence at the most formative stage of their medical education. The same was true of the remainder of Turnbull's teaching activities. It was his practice for instance in the early years of his directorship to demonstrate daily in the post-mortem room any material of particular interest and these demonstrations were much appreciated and well attended; but from 1925 he ceased to do this, leaving these demonstrations to his assistants. And when that same year it was decided, after consultation with Panton, to give the students about to enter on their clinical studies a short introductory course in pathology so that they should have some basic knowledge of the subject and at least understand its language, Turnbull delegated this teaching to the Junior Assistant Director, at that time Dr Bratton. There were, no doubt, excellent reasons for these decisions, with this I am not really concerned. The point I wish to make is that from 1926, that is for more than twenty years before he retired, Turnbull had little or nothing directly to do with the training of the undergraduates at the London Hospital. And it follows from this that the influence which he had as a teacher, and we have seen how important and beneficial this proved for pathology, made itself felt at a higher level, on his junior staff and on the numerous postgraduates who came to work as assistants in his department. This he achieved more by the individual guidance that he gave to those working with him and by the example that he set them than by anything else. His unswerving honesty, his meticulous attention to detail and his unrelenting pursuit of truth made of him a legendary figure dominating the activities of his institute and acting as an inspiration to even the most junior of his colleagues, technical and qualified alike. Nothing but the best, nothing but the truth was acceptable to Turnbull and all who were privileged to work with him were expected to aim at the same high standard and made the attempt if not always the grade. The list of those who held the post of Pathology Assistant at the London Hospital in Turnbull's time is a long one. Some of these individuals never had the intention of making a career in pathology, they wished to work with Turnbull so as better to prepare themselves for a career in some clinical specialty; the names of many distinguished men are to be found amongst them. The majority, however, came to Turnbull's department with specialization in pathology in view and during the years that he held the post of director forty made this choice, several reaching eminence in this specialty. Four of the chairs of pathology in this country have been held by pupils of Turnbull; he was himself succeeded by one of his own training, Professor Dorothy Russell. And when one recalls that in addition to the regular pathology assistants recruited from amongst those who graduated at the London Hospital, many came from all over the world to work voluntarily as assistants to Turnbull it will be realized how widespread his influence has been.

When one comes to consider Turnbull's researches it is difficult at times to avoid a feeling of disappointment. This is not occasioned by any lack of quality in his published work, because Turnbull was incapable of that; it is the thought of the time which he must have devoted, at the expense of his planned personal research, to the compilation of notes on the pathological findings in cases which were being written up by his clinical colleagues. Often enough these notes appeared under Turnbull's initials as addenda to the papers which they illuminated, his name not appearing with those of the other authors, but Turnbull was probably indifferent to this, for he was the most generous and unselfish of workers, caring little who received credit for the work so long as its excellence and accuracy were assured. And it is not suggested that these activities were either unimportant or necessarily irksome to Turnbull, in fact at times they provided him with the greatest of pleasure. This was true for instance of his collaboration with Dr Donald Hunter in the latter's study of calcium and phosphorus metabolism and the part played in this by the parathyroids. But the fact remains that out of the 82 scientific publications listed in Turnbull's bibliography 24, nearly a third, were initialled reports prepared by him for clinical colleagues. And we also know that he had to lay aside his studies on bone pathology for completion when he retired, an intention which, unfortunately, remained unrealized owing to ill health. It is probable, therefore, that this was a factor which contributed to the restriction of Turnbull's scientific output. Another was his extreme reluctance to commit himself on paper. By nature ultra-cautious he had a fear of falling into error which proved most inhibiting when it came to writing a paper. Each statement had to be weighed most carefully and every phrase searched for ambiguity of meaning, added to which he was a stylist who tolerated nothing but the most polished prose. All things considered it is hardly surprising that his output of scientific publications should have been somewhat limited. None the less amongst those papers that he did publish, either alone or with one or other of his colleagues, is to be found original work of outstanding merit. Here the pride of place belongs to his identification of post-vaccinal encephalomyelitis. Between seeing his first case in 1912 and obtaining further material there was an interval of ten years. But in 1922, having satisfied himself of the connexion between vaccination and the perivascular demyelinating lesions which he had observed in the central nervous system, he reported the matter to the Ministry of Health. Further cases having come to light the Ministry, early in 1924, appointed a Commission to investigate the matter, at the same time putting a ban on publication. In the meantime cases were observed by Bastiaanse in Holland, who published an account of his findings in 1925. Since Turnbull's paper on the subject, published jointly with McIntosh, did not appear until the next year priority of publication went to the Dutch author. Two years later Turnbull (1928) drew attention to the essential similarity of this condition and the encephalomyelitic complication of the exanthemata and the correctness of this is recognized today by grouping these conditions together with the common designation of post-infection encephalomyelitis. Turnbull evidently found inspiration in his association with his colleague McIntosh because two further important pieces of work came from this joint authorship. In 1913, four years after Landsteiner and Popper (1909) demonstrated the viral aetiology of acute anterior poliomyelitis, Turnbull and McIntosh transmitted the disease to monkeys with material from a fatal case and made a histological study of the lesions in the experimental disease. And in 1920 they reported the results of their investigation of encephalitis lethargica which made its appearance as a 'new' disease in this country and parts of Europe in the later years of the first World War. Their claim to have transmitted the condition to monkeys remains unconfirmed; in fact the aetiology of encephalitis lethargica has yet to be settled and the opportunity of doing so now is remote since the disease seems to have disappeared. Turnbull's association with Schmorl early in his career gave him a special interest in bone pathology which was to continue to the end, though here again his published papers are few. In 1922 he published his researches on the inflammatory changes in the long bones found in congenital syphilis. This was followed a few years later by two papers on foetal rickets, and it was about this same period that Turnbull was associated with Donald Hunter in the classical researches on the metabolism of calcium and phosphorus in generalized disease of bones to which reference has already been made. And his last publication of all was also in the field of bone pathology: the description with one of his old pupils, S. L. Baker, of a new collagen disease of bone matrix. That was in the nature of a preliminary communication which owing to ill health Turnbull was unable to pursue; it was left to Baker to do so. Amongst the more important of his other scientific writings are papers on the cardiovascular system, 'Alterations in arterial structure and their relation to syphilis', which appeared just before the commencement of the first World War, a paper on intracranial aneurisms some four years later and finally the report of a discussion on obliterative vascular disease in which he took part in the mid-thirties. According to those who are better qualified to judge than I am, the first of these papers is a classic the value of which is undimmed by the passage of time. The accuracy of the Wassermann reaction as checked by post-mortem findings, the pathology of human psittacosis and T.N.T. poisoning are other subjects on which Turnbull worked and wrote and it is clear that although he may not have been a prolific writer he made valuable contributions to many aspects of pathology.

If Turnbull was a reluctant writer he was certainly no better where the spoken word was concerned. He rarely read papers before scientific societies or took part, officially or unofficially, in the discussions they organized. It is true that he was a foundation member of the Pathological Society of Great Britain and Ireland and was to be seen at its meetings from time to time and he was also a member of the Medical Research Club of London and found pleasure in its activities. But generally speaking he held himself aloof from meetings. This has been attributed to the fact that he suffered acutely from migraine and that attendance at meetings was apt to precipitate an attack of this incapacitating condition. And although this was undoubtedly the case it seems highly probable that the almost morbid fear of committing himself to some statement which might subsequently prove untrue, which was such a brake on his scientific writing, was also responsible for preventing Turnbull from playing the full part in scientific discussions which his considerable knowledge entitled him to do. It is also more than probable that this attitude of Turnbull to activities outside his institute and the medical school of which it formed a part, was responsible for the relatively few honours that came his way. In 1919 the University of London conferred on him the title of Professor of Morbid Anatomy and he was elected a Fellow of the Royal College of Physicians in 1929 under bye-law 39B. Election to the Fellowship of the Royal Society came ten years later and in 1945, two years before he retired, Oxford University conferred on him the honorary degree of D.Sc. That same year the *Journal of Pathology and Bacteriology*, to commemorate his 70th birthday, produced a special number contributed by his pupils and dedicated to him. When he retired he was made Emeritus Professor of Morbid Anatomy by the University of London. Turnbull was no seeker after honours, in fact he was largely indifferent to such things. But his election to the Royal Society and the receipt of an honorary D.Sc. from his old university gave him very real pleasure, and rightly so, since it was a recognition of the successful fulfilment of the task which he had set himself when he elected to make a career in pathology.

So far we have been concerned with Turnbull as a man of science; what of the other characteristics which contributed to his make-up? His tall gaunt frame with his somewhat unbending attitude and severe mien suggested an austerity which was at variance with the generosity and kindness of heart which lay beneath. He was a shy man who found it difficult, except perhaps in writing, to express the full depth of his feelings and he was at times misjudged on first meeting. But those who were privileged to know him well realized that he was a man of generous intent who enjoyed companionship and good conversation and who was blessed with a real sense of humour. Perhaps the greatest tribute to Turnbull as a man was the affection and devotion which he inspired in all who worked with him. For them he was 'The Chief' whose constant care was for their welfare and happiness and they were secure in the knowledge that he was always available to give them the benefit of his advice. This was as true of his technical staff as of his professional colleagues; requests for his help never went unheeded.

It might easily be assumed from what has been said that Turnbull had no interests outside the subject he professed, but that would be quite incorrect, for he had many hobbies which he pursued with almost the same zeal that he devoted to morbid anatomy. His interest in birds, inculcated in his boyhood by the Scottish naturalist, William Evans, blossomed into bird watching when he went up to Oxford and he continued to take an active interest in this pursuit for the rest of his life; as might have been expected Turnbull kept exact notes of all that he observed. He was also an expert angler and many of his holidays were spent fishing for salmon or trout; incidentally he wrote a series of papers on the scales of the salmon which appeared in *The Field* in 1909-10. Geology was another of his holiday interests and at one period of his life his kit for a country holiday always included a geological hammer. He played golf with more than average skill; in fact I have always understood that his handicap, which was never above single figures, had at one time been down to scratch. His garden at Woking provided him with relaxation at week-ends and since the soil there is an acid one he was able to indulge to the full his particular fancy in flowering shrubs the azalea and rhododendron; at the Chelsea Flower Show exhibits in this class were always carefully inspected by him. And when one adds to this list of recreations that of water-colour painting which Turnbull took up in his advancing years, it will be realized how wide his interests were.

Illness had proved a handicap throughout his life. His postgraduate training was twice interrupted by illness and migraine, which in his undergraduate days was so severe as to raise the question of cerebral tumour in differential diagnosis, had a considerable influence on the pace and pattern of the earlier part of his scientific career and was largely responsible for his becoming something of a recluse; fortunately the attacks became less severe with advancing years as is usually the case with this disease. A scratch of his right forefinger when bird nesting in 1923 opened the way for infection which generalized with the production of a severe bacteraemia; recovery left him with ankylosis of the interphalangeal joints of that finger. And in his later years he suffered from pulmonary tuberculosis contracted, it is supposed, in the post-mortem room. Added to these misfortunes the death in 1933 of his wife, to whom he was devoted, affected him deeply.

Owing to the outbreak of war in 1939 Turnbull's retirement was postponed for six years and during the war he took up residence in the London Hospital regardless of the danger of living in that part of London, in order better to cope with the task of running his department with a much depleted staff. Although Dr W. W. Woods, who had been his devoted assistant director since 1920, gave him every possible support this task proved a great drain on Turnbull's physical resources and he aged manifestly. And when he came to retire in 1947 he found that he no longer possessed the energy for all the things which he had looked forward to doing in retirement. Unhappily also his retirement was marred by a series of illnesses some of which necessitated treatment in hospital and he returned to the London Hospital on several occasions, but these times as a patient. His eightieth birthday found him there for the purpose of a further radiological examination of his chest and some five months later he died peacefully in the hospital which he had served so well and faithfully. Turnbull had four children, three sons and a daughter, all of whom survive him.

SAMUEL BEDSON

BIBLIOGRAPHY

1904. Bilateral loss of post-central cortex, apparently congenital, in an adult. *Brain*, **106**, 209-251.

1908. Two cases of bone and marrow in the suprarenal capsule, a contribution to the study of heteroplastic bone-formation. *Arch. Path. Inst. Lond. Hosp.* **2**, 75-114.

1908. (With R. WORTHINGTON.) Regeneration of the liver. *Arch. Path. Inst. Lond. Hosp.* **2**, 35-58.

1908. (With R. WORTHINGTON.) Three cases illustrating the transition from regeneration to carcinoma in cirrhosis of the liver. *Arch. Path. Inst. Lond. Hosp.* **2**, 59-74.

1908. (With R. WORTHINGTON.) Two cases of carcinoma arising primarily in a bronchus. *Arch. Path. Inst. London. Hosp.* **2**, 163-175.

1908. (With R. WORTHINGTON.) A case illustrating the importance of microscopic examination of the prostate in tumours of obscure origin. *Arch. Path. Inst. Lond. Hosp.* **2**, 175-178.

1908. (With F. S. KIDD.) A contribution to the histogenesis of Hodgkin's disease: a study of two cases. *Arch. Path. Inst. Lond. Hosp.* **2**, 130-156.

1908. (With F. N. STANDISH.) Bilateral ovarian tumours secondary to scirrhous carcinoma of the stomach. *Arch. Path. Inst. Lond. Hosp.* **2**, 179-187.

1909-10 The scales of salmon. *The Field, The Country Gentleman's Newspaper*, 1909, 11, 18, 25 December, and 1910, 10 December.

1910. (Necropsy and histology for J. PARKINSON.) A case of pernicious anaemia terminating in acute diabetes. *Lancet*, **2**, 543-546.

1910. (With H. LETT.) A case of benign cyst of the humerus. *Lancet*, **2**, 1202-1208.

1911. Mesaortitis with aortic incompetence in father and daughter. *Trans. Med. Soc. Lond.* **34**, 240-245.

1911. Mesaortitis in a child aged seven. *Trans. Med. Soc. Lond.* **34**, 245-247.

1911. Ectopic chorionepithelioma. *Trans. Med. Soc. Lond.* **34**, 247-251.

1912. Obituary (initialed H.M.T.): Dr Charles Umpherston Aitchison. *Lond. Hosp. Gaz.* No. 160, 141.

1912. (With T. Thompson.) Primary occlusion of the ostia of the hepatic veins. (Includes pathogenesis of occlusion.) *Quart. J. Med.* **5**, 277-296.

1912. (Pathological report for H. Macnaughton Jones.) Case of ganglion neuroma (partly embryonic in structure) of the mesentery. *Proc. R. Soc. Med.* **5**, (Obst. & Gyn. Sect.), 287-299.

1912. (Necropsy and histology for T. Thompson.) A case of subacute combined degeneration of the spinal cord demonstrating the nature of the afferent impulses in the posterior columns. *Brain*, **34**, 510-534.

1913. (With J. McIntosh.) Transmission to monkeys of virus obtained from English cases of poliomyelitis. *Lancet*, **1**, 512-518.

1913. A case of familial malformation in a fowl's head. *Biometrika.* **9**, 538-539.

1913. (With F. Kidd, S. G. Scott and E. C. Lindsay.) Pyeloradiography: a clinical study. *Proc. R. Soc. Med.* **7** (Surg. Sect.), 16-40.

1913. (Pathological report for J. H. Sequeira.) Case of multiple idiopathic pigment sarcoma (so-called) of Kaposi. *Proc. R. Soc. Med.* **7**, (Derm. Sect.) 7-10.

1914. (With J. H. Sequeira.) Sarcoma of the skin etc., secondary to tumour of the foot. *Brit. J. Derm.* **26**, 411-416.

1915. (With F. S. Jones.) Sclerodermis guttata. *Brit. J. Derm.* **27**, 450-464.

1915. Alterations in arterial structure and their relation to syphilis. *Quart. J. Med.* **8**, 201-254.

1915. Reports on morbid anatomy in *Yellow fever commission* (*West Africa*). *Reports on questions connected with the investigation of non-malarial fevers in West Africa*, **1**, 196-206, 219-222 etc. London, H.M. Stationery Office.

1916. (Necropsy and histological findings for F. P. Weber.) Sequel to the case of chronic splenomegaly of uncertain origin, with persistent leucopenia, shown on January 12 1912. *Proc. R. Soc. Med.* **9** (Clin. Sect.), 1-6.

1916. (With C. Allbutt.) Notes on a case of syphilis of the aorta. *Lancet*, **1**, 1033-1034.

1916. (Necropsy with measurements and weights for R. Hutchison.) Report on a case of hemihypertrophy. *Proc. R. Soc. Med.* **9** (Sect. Dis. of Child.), 66-68.

1917. Toxic jaundice in munition workers. *Proc. R. Soc. Med.* **10** (Part I, General Reports), 47-67

1918. Intracranial aneurysms. *Brain*, **41**, 50-56.

1920. The accuracy of Wassermann tests. I. The Wassermann test applied before death. *Med. Res. Comm. Spec. Rep. Ser.* No. 47. London: H.M. Stationery Office.

1920. Results of the examination of tissues from eight cases of death following injections of salvarsan. *Med. Res. Comm. Spec. Rep. Ser.* No. 55, **1**, 5-43. London: H.M. Stationery Office.

1920. (With J. McIntosh.) The experimental transmission of encephalitis lethargica to a monkey. *Brit. J. Exp. Path.* **1**, 89-102.

1920. Glycogenic infiltration of the liver of laboratory rabbits. *Brit. J. Exp. Path.* **1**, 310-316.

1921. (With F. H. Jacob, J. A. Arkwright and G. M. Dobrashian.) A case of human glanders. *Brit. J. Derm. & Syph.* **33**, 39-55.

1921. (Histological and complete pathological reports for P. N. Panton and H. E. Bates.) T.N.T. poisoning and fate of T.N.T. in the animal body. *Med. Res. Counc., Spec. Rep. Ser. No.* 58. Section V, 66-70. London: H.M. Stationery Office.

1921. (With J. H. Sequeira.) Multiple carcinoma of the skin: 'Precancerous dermatosis of Bowen'. *Brit. J. Derm. & Syph.* **33**, 173-181.

1921-22. Contribution to discussion on syphilitic affections of the stomach. *Proc. R. Soc. Med.* **15** (Occasional Lectures), 9-11.

1922. Congenital syphilitic inflammation of the long bones. *Lancet*, **1**, 1239-1241.

1922. Obituary (not signed and not initialed): Dr Henry Wynyard Kaye, 1875-1922. *Lond. Hosp. Gaz.* **25**, 130-131.

1923. Pathological report in the serum diagnosis of syphilis. *Med. Res. Counc., Spec. Rep. Ser. No.* 78. Appendix II, 96-99. London: H.M. Stationery Office.

1924. (With A. W. Ellis.) Hyperglycaemia and glycosuria in acromegaly. Pathological reports in appendix. *Lancet*, **1**, 1200-1203. Correction of final sentence in appendix, *Lancet*, **1**, 1264.

1926. (With J. McIntosh.) Encephalomyelitis following vaccination. *Brit. J. Exp. Path.* **7**, 181-222.

1927. The histology of mycosis fungoides. In *Diseases of the skin.* By J. H. Sequeira, pp. 580-582. London.

1927. (Histological report with necropsy by A. B. Bratton for D. Forrest.) A case of transient paraplegia in Hodgkin's disease. *Lancet*, **2**, 809-810.

1928. Encephalomyelitis in virus diseases and exanthemata: the histology of post-vaccinal encephalitis. *Brit. Med. J.* **2**, 331-334.

1928. (Observations not previously published in A. S. MacNalty.) Lymphadenoma with relapsing pyrexia. *Ministry of Health Rep.* No. 50. London: H.M. Stationery Office.

1929. (Description of kidney for L. J. Witts.) A note on blood transfusion with an account of a fatal reaction. *Lancet*, **1**, 1297-1299.

1929. (With W. R. Brain and D. Hunter.) Acute meningoencephalomyelitis of childhood. *Lancet*, **1**, 221-227.

1929. Post-vaccinal encephalomyelitis. Report to the Coroner. *Lancet*, **2**, 43.

1929. (With G. L. S. Konstam.) Syphilis of the lung with pulmonary arteritis (Ayerza's disease). *Lancet*, **2**, 756-761.

1929. (Pathological report for D. Hunter.) Hyperparathyroidism (Hyperfunction of a parathyroid tumour in a case of generalized osteitis fibrosa). *Proc. R. Soc. Med.* **23**, 227-234 (Med. Sect., 27-34).

1929. (Lymphosarcoma in R. A. Rowlands and S. L. Simpson.) Chronic lymphatic leukaemia with aleukaemic phase. *Proc. R. Soc. Med.* **23**, 218-222 (Med. Sect., 218-222).

1929. (Pathological report for W. R. Brain and F. B. Byrom.) Hypercholesterinaemia. *Proc. R. Soc. Med.* **23**, 222-224 (Med. Sect., 22-24).

1929. (Pathological report for D. Hunter and W. Evans.) Gaucher's disease thirteen years after splenectomy. *Proc. R. Soc. Med.* **23**, 224-227 (Med. Sect., 24-27).

1930. Report upon the histology of the bones of the first case. *J. Path. Bact.* **33**, 332-338. (A separate report following J. P. Maxwell. Two cases of foetal rickets. *J. Path. Bact.* **33**, 327-332.)

1930. (Initialed observations upon the pathology for D. Hunter.) Calcium and phosphorus metabolism: Goulstonian Lectures. *Lancet*, **1**, 897-904, 947-957, 999-1008.

1930. (Pathological reports for R. Hutchison, R. A. Rowlands and S. L. Simpson.) A study of psittacosis, *Brit. Med. J.* **1**, 633-646.

1931. (With M. Young.) An analysis of the data collected by the status lymphaticus investigation committee. *J. Path. Bact.* **34**, 213-258.

1931. (Initialed pathological report upon tissues removed at operation, for D. Hunter.) Hyperparathyroidism: generalized osteitis fibrosa with multiple osteoclastomata. *Proc. R. Soc. Med.* **24**, 486-496 (Clin. Sect., 40-50).

1931. A Jenner stain for sections. *J. Path. Bact.* **34**, 277-279.

1931. (With O. Leyton and A. B. Bratton.) Primary cancer of the thymus with pluriglandular disturbance. *J. Path. Bact.* **34**, 635-660.

1931. (With D. Hunter.) Observations upon the bones, the parathyroid tumours and normal parathyroid glands in Hyperthyroidism: generalized osteitis fibrosa. *Brit. J. Surg.* **19**, 203-284.

1932. Foetal rickets. The bones in Professor Maxwell's third case of foetal rickets or osteomalacia. Part II. *J. Path. Bact.* **35**, 422-440. (Part I. Clinical and pathological. J.P. Maxwell and C. H. Hu, J. *Path. Bact.* **35**, 419-422.)

1932. (Pathological reports for T. G. Reah.) Cinchophen poisoning. *Lancet*, **2**, 504-509.

1932. Obituary (initialed H.M.T.). In memoriam Christian Georg Schmorl, 1861-1932. *J. Path. Bact.* **35**, 982-985.

1932. (Pathological reports (pp. 669-670 and 675-677) in: J. Bennett, D. Hunter and J. M. Vaughan.) Idiopathic steatorrhoea (Gee's disease). *Quart. J. Med.* (New Series), **1**, 603-677.

1933. (Pathological report in: R. S. Aitken.) Atypical teratoma and spheroidal-celled carcinoma of one germinal gland associated with eunuchoidism in an apparent female. *Lancet*, **2**, 1252-1255.

1934. (With J. M. Vaughan.) *The anaemias.* Oxford University Press, Ed. 1.

1935. (Pathological reports for D. Hunter.) Studies in calcium and phosphorus metabolism in generalized diseases of bones. *Proc. R. Soc. Med.*, **28**, 1619-1638 (Clin. Sect., 55-74).

1936. (With J. M. Vaughan.) *The anaemias.* Oxford University Press, Ed. 2.

1936. Contribution to discussion on obliterative arterial disease as it affects the limbs. *Proc. R. Soc. Med.* **29**, 875-877 (Med. Sect., 51-53).

1937. (With W. Evans.) The newer electrocardiogram denoting right bundle branch block. *Lancet*, **2**, 1127-1131 and (histological report) 1184-1186.

1937. (Pathological report for V. W. Dix.) Grawitz tumour of unusually low malignancy. *Proc. R. Soc. Med.* **30**, 459-463 (Sect. Urol., 33-37).

1937. (Report upon six necropsies for R. A. Rowlands and S. L. Simpson.) Gonococcal endocarditis. *Brit. J. Vener. Dis.* **13**, 215-237.

1938. (With W. R. Brain. Pathological report on the ocular muscles and thyroid glands.) Exophthalmic ophthalmoplegia. *Quart. J. Med.* (New Series), **7**, 293-323.

1938. (With A. Burrows.) Cutis hyperplastica (Ehlers-Danlos syndrome). *Brit. J. Derm. Syph.* **50**, 648-652.

1939. (Report upon the bone and marrow for J. M. Vaughan and C. V. Harrison.) Leukoerythroblastic anaemia and myelosclerosis. *J. Path. Bact.* **48**, 339-352.

1940. (Pathological reports for B. H. Page and C. Wilson.) Acute mercurial poisoning after cystoscopy. *Lancet*, **1**, 640-643.

1941. Obituaries (initialed H.M.T.). William Bulloch, 1868-1941. *Brit. Med. J.* **1**, 341-342. Professor William Bulloch, M.D., LL.D., F.R.S., *Lond. Hosp. Gaz.* **44**, 95-98.

1942. (With R. A. Rowlands, S. L. Simpson and D. S. Russell.) The skeletal and other changes found in a case of suprasellar cyst of Rathke's pouch. *Brit. J. Surg.* **29**, 304-319.

1944. Obituary (initialed H.M.T.). George Bertram Bartlett. *Lancet*, **2**, 61-62. Slightly fuller in *Lond. Hosp. Gaz.* **47**, 190-191.

1945. Addendum: the nature of the substance deposited in the spleen and liver of the mice in Dr. L. D. Parson's experiments. *J. Path. Bact.* **57**, 18-20.

1949. (Pathological report for Sir E. Holland.) A case of transplacental metastasis of malignant melanoma from mother to foetus. *J. Obst. Gynaec. Brit. Empire*, **56**, 529-536.

1950. (With S. L. Baker.) Two cases of a hitherto undescribed disease characterized by a gross defect of the collagen of the bone matrix. *J. Path. Bact.* **62**, 132-134.

H. Weyl

HERMANN WEYL

1885-1955

Hermann Weyl was born on 9 November 1885, the son of Ludwig and Anna Weyl, in the small town of Elmshorn near Hamburg. When his school-days in Altona ended in 1904 he entered Göttingen University as a country lad of eighteen, and there remained (except for a year at Munich), first as student and then as Privatdozent, until his call to Zurich in 1913. Of these days he said (in the obituary of Hilbert for this Society, 1944), 'Hilbert and Minkowski were the real heroes of the great and brilliant period which mathematics experienced during the first decade of the century in Göttingen, unforgettable to those who lived through it. Klein ruled over it like a distant god, "divus Felix", from above the clouds.' Among those nearer to his own age whom he found there were Carathéodory and Harald Bohr, Courant, Zermelo, Erhard Schmidt.

While still a schoolboy he had picked up in his father's house an old copy of the *Critique of Pure Reason*, and absorbed with enthusiasm Kant's thesis of the *a priori* nature of Euclidean geometry. But in Göttingen Hilbert had just completed his classical work on the foundations of geometry, with its host of strange 'counter'-geometries. Kantian philosophy could not survive this blow: Weyl transferred his allegiance to Hilbert. 'I resolved to study whatever this man had written. At the end of my first year I went home with the "Zahlbericht" under my arm, and during the summer vacation I worked my way through it—without any previous knowledge of elementary number theory or Galois theory. These were the happiest months of my life, whose shine, across years burdened with our common share of doubt and failure, still comforts my soul.' (133, 1944.)

In spite of the great variety of mathematical stimulation of the Göttingen years, this was the only period of comparable length in which he devoted himself to a single branch of mathematics—analysis, and to a single theme, the problems that arose naturally out of his dissertation, on singular integral equations. Towards the end of this period two causes combined to turn his attention to wider fields. First, in the session 1911-1912 he lectured on the theory of Riemann surfaces, and was led by his sense of the inadequacy of existing treatments to plunge deep into the topological foundations. Secondly, in 1913 he accepted the offer of a chair at the Institute of Technology in Zurich, where his colleague for one year was Einstein, who was just then discovering the general theory of relativity. Weyl was soon launched on the series of papers on relativity and differential geometry which culminated in the book *Raum-Zeit-Materie*. Later still this work led on, through his analysis and generalization of the Lie–Helmholtz space-problem, to his third great theme, the representation theory of the classical groups, and its application to quantum theory.

In the decade 1917-1927, he was at the height of his powers. A stream of papers appeared, not only on his main themes, but on any mathematical topics that interested him—and that meant in almost all parts of mathematics. (A glance at the papers listed in the bibliography under the year 1921 will give some impression of the spread of his interests at this time.) He was ready to defend this universality, this refusal to put all his effort into making steady and systematic progress in one field. 'My own mathematical works were always quite unsystematic, without method or connexion. Expression and shape are almost more to me than knowledge itself. But I believe that, leaving aside my own peculiar nature, there is in mathematics itself, in contrast to the experimental disciplines, a character which is nearer to that of free creative art. For this reason the modern scientific urge to found Institutes of Science is not so good for mathematics, where the relationship between teacher and pupil should be milder and looser. In the fine arts we do not normally seek to impose the systematic training of pupils upon creative artists.' (*Rückblick*, 1955.)

The years at Zurich were happy ones, during which, he says, the worst that happened to disturb his peace was a series of offers of chairs by foreign universities; 'for such decisions worried me'. In the *Rückblick* he tells amusingly how, when he received the first invitation to Göttingen, to succeed Klein in 1923, he walked his wife Hella round and round a block in Zurich till nearly midnight, then jumped on the last tram to telegraph acceptance—and refused. The second invitation, to succeed Hilbert in 1930, he accepted, after still more painful hesitations. But his short stay as professor in Göttingen was clouded over by the threat of coming political events. In 1933 he decided that he could not stay in Germany after the dismissal of his colleagues by the Nazis, and he accepted an offer of permanent membership of the Institute for Advanced Study, then newly founded in Princeton. There he worked as a member till his retirement in 1951, and he remained an emeritus member till his death in 1955, spending half his time there and half in Zurich. Of the Institute he said that it is the finest workshop for a mathematician that it is possible to imagine.

He married in 1913 Helene Joseph, the daughter of a doctor in Ribnitz in Mecklenburg, and there were two sons of the marriage. All who were visitors at the Weyls' house in Mercer Street will remember her charm and gaiety. She shared to the full his taste for philosophy and for imaginative and poetical literature, and was the translator of many Spanish works, including the writings of Ortega y Gassett, into German. She died in 1948.

In 1950 he married Ellen Bär, born Lohnstein, of Zurich, and from that time had the happiness of spending half of each year in Zurich. He died suddenly, of a heart attack, on 9 December 1955.

The last public event of his life was the seventieth birthday gathering, at which he was presented with a volume of 'Selecta' from his own works. A wider circle of his friends had a last happy glimpse of him at the Amsterdam Congress in 1954, where he delivered the address on the work of the Fields Medallists (Kodaira and Serre), a *tour de force* which showed him, in his sixty-ninth year, well abreast of those new theories which have changed the face of mathematics in the last twenty years.

Few mathematicians have left so clear an impression of themselves in their work. His life-long interest in philosophical problems, and his conviction that they cannot be separated from the problems of science and mathematics, has left its mark everywhere in his work. In the last year of his life he wrote a brief philosophical autobiography, which he called 'Erkenntnis und Besinnung', a title which he explained in these words. 'In the intellectual life of man there can be clearly distinguished two domains: the domain of *action* (Handelns), of shaping and construction to which active artists, scientists, technicians and statesmen devote themselves; and a domain of *reflexion* (Besinnung) of which the fulfilment lies in insight, and which, since we struggle in it to find the *meaning* of our activity, is to be regarded as the proper domain of the philosopher.' The essay itself traces with affectionate detachment the philosophical progress of the young Weyl, from Kant through Husserl's 'Phenomenology' and Fichte's Idealism to his discovery in 1922 of the medieval mystic Eckehart, who gave him for a time 'that access to the religious world which I had lacked ten years earlier. . . . But my metaphysical-religious speculations, aroused by Fichte and Eckehart, never came to a clear conclusion; that was in the nature of things.' He turned, under the stimulus of writing his book on the philosophy of science (1927) to the astringent pages of Leibniz. 'Auf den metaphysischen Hochflug folgte die Ernüchterung.'

In mathematical logic, too, things seemed a little less sure at the end of his life than at the beginning, 'the world becomes strange, the pattern more complicated', but he held steadily to his view that postulation cannot replace construction without the loss of significance and value. This belief he held so seriously that he deliberately kept away throughout his life from those mathematical theories which make essential and systematic use of the Axiom of Choice.

The literary graces with which he liked to adorn his work gave it an unmistakable flavour. Who else, in the austere pages of the London Mathematical Society, would sum up the outcome of Minkowski's 'unexpected difficulties' in the geometry of numbers in the words: 'But of him it might be said, as of Saul, that he went out to look after his father's asses, and found a kingdom'? (127, 1942). And who else would have found in *Der Rosenkavalier*, of all places, an apt and serious quotation for his chapter on 'Ars Combinatoria' (genetics)? It was a particularly cruel fate that imposed upon him 'the yoke of a foreign tongue that was not sung at my cradle'. Under this handicap he did not resign himself to writing in a drab and timid style, but used his adopted language as boldly and colloquially as his own—

sometimes, it is true, with results which were surprising as well as pleasing.

The mathematical form of his presentations was even more characteristic than his literary style. His strong preference for arguments that stem from the central core of the problem, rather than verifications—even easy verifications—by computation, and his liking for pregnant verbal statements where others might use symbols, more easily seized by the mathematician's eye, sometimes made close demands on the reader's attention, but the reward was doubly great when the passage was understood. But his absorption in conceptual analyses and general theories never extinguished his zest for formal mathematical detail. He considered examples to be the life-blood of mathematics, and his books and papers are full of them. In the obituary of Hilbert he mentions with admiration the many examples by which Hilbert illustrated the fundamental theorems of his algebraic papers—'examples not constructed *ad hoc*, but genuine ones worth studying for their own sake!' The *dictum* of Hilbert on the subject, in *Mathematical Problems*: 'The solution of problems steels the forces of the investigator: by them he discovers new methods and widens his horizon. One who searches for methods without a definite problem in view is likely to search in vain:'—this he took as a precept in his own mathematical life.

He was indeed not only a great mathematician but a great mathematical writer. His style was leisurely by modern standards, but it had a wonderful richness of ideas. His discoveries will surely not only long survive as mathematics, but will be read in his own incomparable accounts of them.

In some brief notes of biographical data, written in August 1955, after enumerating his honorary doctorates and membership of academies, he wrote: 'Von allen Ehrungen betrachte ich als die auszeichnendste die schon 1936 erfolgte Aufnahme unter die verhältnismässig kleine Zahl der auswärtigen Mitglieder der Royal Society (London).'

MATHEMATICAL AND SCIENTIFIC WORK

(This part of the Memoir has been written with the collaboration of H. Davenport, F.R.S., P. Hall, F.R.S., G. E. H. Reuter and L. Rosenfeld, who have supplied the material for passages dealing with their subjects. The range and extent of Weyl's work was so great that, if a mere catalogue was to be avoided, it was necessary to select certain parts of it for discussion. This has entailed omitting many important but isolated papers.

The arrangement is roughly by subjects, which have been introduced in approximately the order in which Weyl first began to work in them, except that mathematical logic is put last. M.H.A.N.)

Throughout his life Weyl continued to write papers from time to time on topics in *analysis*, but the long series of papers (1908-1915) in which (following Hilbert's precept) he applied the new theory of integral equations to eigenvalue problems of differential equations establishes most clearly his stature as an analyst.

The papers written in this period fall into two groups. In the first (6, 7, 8, 1909-1910) the theory of singular eigenvalue problems is developed for ordinary differential equations. In the classical Sturm-Liouville problem the equation

$$\frac{\mathrm{d}}{\mathrm{d}x}\left(p(x)\frac{\mathrm{d}u}{\mathrm{d}x}\right)+(\lambda-q(x))u=0 \tag{1}$$

is considered on a finite interval with one boundary condition at each end. There is then a discrete sequence of eigenvalues and eigenfunctions, and any smooth function satisfying the boundary conditions can be expanded in a series of eigenfunctions. Weyl took up the 'singular' case, where either the interval is semi-infinite, or $p(x)$ and $q(x)$ are allowed to become singular at one end. He proved the following alternative for the boundary condition at the singular end. Either the solutions are of integrable square for every λ, and thence a boundary condition must be imposed and the expansion takes the classical series form; or if for at least one λ not all solutions are of integrable square, no boundary condition can be imposed. In this case the expansion usually involves Stieltjes integrals with respect to the eigenvalue parameter λ, and the eigenfunctions occurring in the expansion may be 'improper', i.e., not of integrable square. Weyl hoped to extend this theory to partial differential equations, but published nothing further on it. A satisfactory extension has indeed only been found quite recently, and the subject is very much alive at present.

The second early group of papers in analysis (12, 15-17, 19, 22, 1911-1915) are on the asymptotic distribution of natural frequencies of oscillating continua, *e.g.* membranes, elastic bodies and electromagnetic waves in a cavity with reflecting walls. The simplest example of such problems is the investigation of the eigenvalues $\lambda_1 \leqslant \lambda_2 \leqslant \dots$ of the equation $\Delta u+\lambda u=0$ in a plane domain D. Weyl proved the remarkable result that the asymptotic behaviour of λ_n depends only on the area, A, of D, and not on its shape, nor on the precise form of the boundary condition, which may, *e.g.*, be $u=0$ or $\frac{\partial u}{\partial n}=0$. The asymptotic value is in any case $\lambda_n \sim 4\pi n/A$. This he proved (when the boundary condition is $u=0$) by reducing $\Delta u+\lambda u=0$ to an integral equation and showing that, if D_0 is split into non-overlapping regions D_1, D_2 and if D_i contains $N_i(t)$ eigenvalues $\leqslant t$, $(i=0,1,2)$ then

$$N_0(t) \geqslant N_1(t)+N_2(t).$$

By covering D by non-overlapping squares the asymptotic relation can then be deduced from the known eigenvalue distribution for a square.

Weyl's outstanding quality as an analyst is already shown by these early papers. In all of them the argument moves by clearly visible steps, each involving difficult work ranging from the estimate of the Green's function near the boundary to delicate questions that arise when the associated homogeneous integral equation has non-trivial solutions. A distinctive character is given to the work by the combination of a professional analyst's technical powers with the deliberate selection of problems of concrete physical interest. In this earliest phase of his work he already considered it a duty to use advances in analysis to solve the problems of natural science.

Apart from his work on almost periodic functions (72, 1927) which is intimately connected with the 'Peter–Weyl Theorem' (see later) the remaining papers which Weyl wrote from time to time on topics in analysis had more the character of occasional pieces, but all bore the mark of his immense skill as an analyst.

In the *theory of numbers* his papers were few in number, but of great influence. The memoir on the uniform distribution of numbers mod. 1 (23, 1916) was of fundamental importance for almost all later work in the analytic theory of numbers. A preliminary note on the subject had appeared in 1914.

The definition of uniform distribution (Gleichverteilung) modulo 1 of a sequence $\alpha_1, \alpha_2, \ldots$ of real numbers is a very simple and natural one. For any γ between 0 and 1, let $A(N,\gamma)$ denote the number of those of $\alpha_1, \ldots, \alpha_N$ which have fractional parts between 0 and γ. Then the condition is that

$$\frac{A(N,\gamma)}{N} \to \gamma \tag{2}$$

as $N \to \infty$, for each γ. The basic theorem of Weyl's memoir is that a sequence is uniformly distributed if and only if, for each integer h other than 0,

$$\frac{1}{N}\sum_{n=1}^{N} e^{2\pi ih\alpha_n} \to 0 \tag{3}$$

as $N \to \infty$. This theorem is so elegant that it seems strange, on looking back, that it was not discovered earlier, especially as the proof is not particularly difficult.

The theorem that, if θ is any irrational number, the sequence $\theta, 2\theta, 3\theta, \ldots$ is uniformly distributed modulo 1, which had already been proved in a variety of ways by others, is an immediate deduction from Weyl's theorem. For from the elementary estimate

$$\left|\sum_{n=1}^{N} e^{2i\pi hn\theta}\right| \leqslant |\sin \pi h\theta|^{-1}, \tag{4}$$

it follows that for each integer h the sum on the left is bounded independently of N, and the condition (3) is satisfied.

A more important deduction was that the sequence $f(1), f(2), \ldots$ is uniformly distributed modulo 1, if $f(x)$ is a polynomial:

$$f(x) = \theta x^k + \theta_1 x^{k-1} + \ldots + \theta_{k-1}x,$$

with at least one irrational coefficient. To establish this, Weyl developed a method of estimating exponential sums of the form

$$S = \sum_{n=1}^{N} e^{2\pi i f(n)}.$$

The resulting estimate, known as *Weyl's inequality*, played (in various forms) a vital part in later work of Hardy and Littlewood, van der Corput, Vinogradov and many others. Weyl's inequality, which includes (4) as a special (but degenerate) case, is as follows:

$$|S|^K < 4^K \left\{ N^{K-1} + N^{K-k} \sum_{m_1=1}^{N} \dots \sum_{m_{k-1}=1}^{N} \min(N, \| \theta k!\, m_1 \dots m_{k-1} \|^{-1}) \right\},$$

where $K = 2^{k-1}$ and $\| \dots \|$ denotes the difference between any real number and the nearest integer. From this Weyl easily deduced that the condition $S/N \to 0$ as $N \to \infty$ for uniform distribution is satisfied provided that θ is irrational, and the method can be extended to give the slightly more general result stated above.

To express Weyl's inequality in a simple and explicit form, it is necessary to make some supposition about the irrational character of one of the coefficients in $f(x)$, and this was done by others, notably Hardy and Littlewood, Landau, and Vinogradov. If we suppose, for example, that

$$\left| \theta - \frac{a}{q} \right| < \frac{1}{q^2},$$

where a and q are relatively prime integers and

$$N^{1/k} < q < N^{1-1/k},$$

then $S = O(N^{1-1/k})$. This result played a vital part in Hardy and Littlewood's work on Waring's Problem, in which Weyl himself took a keen interest (see his note 44, 1921). It was also the essential tool for the estimation of the Riemann zeta-function, a task to which Weyl also contributed (42, 1921). It was not until 1936 that a more effective method of estimating exponential sums of the above kind was developed by Vinogradov, and even so his method gives an improvement only for large values of k.

Enough has been said to make clear the importance of the memoir of 1916, though only part of its contents have been indicated. It remains a paper which can be read and re-read with profit today.

Weyl returned to the theory of numbers in 1939-1940 with his book *Algebraic theory of numbers* (119, 1940) based on lectures he gave at Princeton in 1938-1939. This is a text-book, but a text-book written in a highly individual style and with a particular theme. Weyl contrasts the relative merits of the two principal methods of developing algebraic number theory: that based on *ideal theory*, due to Dedekind, and that based on *divisor theory*, due to

Kronecker and Hensel. The two are equivalent in their effects, but Weyl gave a preference to the second, for reasons which he explains. It is interesting to observe that in spite of his many other interests Weyl still retained his mastery of the detail of a subject of such a formidably technical nature as algebraic number theory.

He also wrote at this time two papers on the geometry of numbers (127, 1942, and 122, 1940 and 1942). The second paper is a sequel to the first (which was sent in in 1939). The two papers are partly of an expository character, and do not contain much that was essentially new, though they clear up some intricate questions concerning arithmetical equivalence in a comparatively simple way and put the matter in a clear light.

In 1913 there appeared *Die Idee der Riemannschen Fläche*. This book, in which Weyl revealed his full powers for the first time, marked the beginning of the widening of his mathematical interests. By its declared subject it belonged to analysis, and indeed it contained a masterly exposition of the classical theory of algebraic and analytic functions on Riemann surfaces, culminating in a proof of the uniformization theorem. But it was the plan, revolutionary at that time, of placing 'geometrical' function theory on a basis of rigorous definition and proof, hitherto enjoyed only by the Weierstrass theory, that gave the book its unique character, and forced Weyl to plunge deep into the *topology* of manifolds. In his Lectures on Algebraic Functions of 1891-1892 Felix Klein had shown that the notion of a Riemann surface need not be tied to the multiply-sheeted coverings of a sphere to which Riemann had confined himself, but could be extended to include any surface provided with *local uniformizing variables* (conformal maps of the members of an open covering on to a circular domain). When Klein delivered his lectures there were no means available of giving exact form to these ideas: the lack of topological notions made it impossible even to define a Riemann surface precisely. In 1910 and 1911 L. E. J. Brouwer published his papers on the topology of simplicial manifolds. Weyl saw at once that here was the basis for an exact treatment of Klein's ideas, with Hilbert's proof of the Dirichlet Principle as the instrument for establishing the existence of differentials on the surface. To these ingredients, which, as might be expected, he modified and simplified to suit his purpose, he added others of his own. In order to prove, as he wished, that the 'analytisches Gebilde' can itself be regarded a surface, he needed a thoroughgoing axiomatic definition of a surface, which should make it clear that the 'points' can be mathematical objects of any kind (in this case pairs of power series). The notion of a *neighbourhood-space*, as a set in which certain subsets are associated with each point as its neighbourhoods, had been introduced by Hilbert in 1902 (*Math. Ann.* **56**, reprinted as Anhang IV to *Grundlagen der Geometrie*), but his definition remained unused and almost unnoticed. Weyl revived and clarified Hilbert's definition, and showed for the first time how it could be applied. The conditions which make the restriction to manifolds were not separated, as they would be today, from the general topological axioms, but the notion of a *topology* as a designated family of subsets, was clearly brought into view. He could now define a *surface* to be a (connected) triangulable 2-manifold, and a *Riemann surface* to be a surface on which, for each point p_0, certain complex-valued continuous functions are designated *regular at* p_0, again subject to suitable axioms. The *analytisches Gebilde* (a notion also here made exact for the first time) is a set of 'function elements', f, i.e., pairs $(t^{\mu}P, t^{\nu}Q)$ where P and Q are power series in t $(P \neq \text{constant})$ and μ, ν are integers. The process of direct continuation having been defined in a natural way (pp. 8 and 9) a topology is set up by taking as a neighbourhood of f_0 the set of all its direct continuations to points in $|t| < \epsilon$, for some $\epsilon > 0$. This topological space is clearly a 2-manifold with 'local uniformizers'. It is proved rather laboriously that it can be triangulated, and is, therefore, a Riemann surface. But it is more than that, since two meromorphic functions on the surface, determined by the elements $(t^{\mu}P, t^{\nu}Q)$, are given as part of the definition. The first main task is to prove, conversely, that on any given Riemann surface there can be constructed a pair of meromorphic functions which make it into an 'analytisches Gebilde'. It is for this purpose that a particularly elegant proof of the Dirichlet Principle is included (§12).

Another of the new ideas which Weyl brought to his task had to wait more than twenty years to be independently rediscovered in more general form by topologists. This was the isolation of the topological part of the proof of the duality between the differentials and the 1-cycles on the surface. The 'curve-functions' introduced in §11 are 1-dimensional *co-chains* on the Riemann surface: the equation $F(\gamma) = f(p_2) - f(p_1)$ on p. 68 states precisely that F is the co-boundary of f, and shows that the symbol $F \sim 0$ has the meaning that is given to it in homology theory. The duality theorem, that the 1-dimensional connectivities derived from cycles and co-cycles are equal, is established in this section.

Still another substantial contribution made in this book to the topology of the subject is the treatment of the covering surface. This notion had been used by Poincaré, but only Weyl's exact definitions and proofs made clear what precisely are the parts played by the topological and the function-theoretic properties.

It is natural that some of these pioneering topological chapters should appear somewhat rugged to modern readers. Weyl himself published a new edition in 1955 in which he got rid of the troublesome triangulation condition, and cast many of the definitions into the clearer forms which forty years of progress in topology had made possible.

It is convenient to mention here another contribution of Weyl to topology, though its connexion with continuous manifolds would have seemed remote when it was published. This was the short series of papers (59, 1923-1924) written in Spanish, on 'combinatorial analysis situs', that is the axiomatic theory of cell-complexes. A good deal of the material was of an expository character and Weyl himself seems to have attached little importance to the papers; but in fact this was the first appearance in the literature of a homology theory based on an axiomatic definition of abstract cell-complexes. Dehn and Heegaard had indeed (in the *Enzyklopädie* article III, AB3 1907) developed a purely combinatorial theory, but their 'spheres' (cell-boundaries) were defined constructively, as complexes obtainable by allowed transformations from simplex-boundaries. Weyl took the opposite course of starting with an undefined boundary operation and imposing (in the form of axioms) such restrictions as were required. His postulate I_n contains, in somewhat involved form, the crucial condition $\partial\partial = 0$, but having his attention fixed from the start on invariance under subdivision, he did not bring out clearly the fact that this relation alone is a sufficient basis for a homology theory. He also considered the constructive method of defining spheres, and in this springtime of topology it was possible for him to say that, although the two methods failed to meet, it was to be hoped that with progressive developments in analysis situs the gap would soon be closed.

Weyl's interest in *general relativity*, and through it in *differential geometry*, began through his giving a course of lectures on the subject in Zurich after the departure of Einstein to Berlin—lectures which were the nucleus from which the book *Raum-Zeit-Materie* grew, through a series of revisions and expansions, to the great treatise of 1923 (5th edition). This book is too well-known to need lengthy description. It gave Weyl his first opportunity to combine discussion of the philosophical questions in which he was so deeply interested with technical mathematics. On the mathematical side, it is distinguished, as might be expected, for the precision of the results. Nowhere else, for example, is there to be found so thorough and exact a discussion of the central orbit, finishing with rigorous inequalities for the maximal and minimal distances—a useful piece of information for discussion of the motion over long periods of time.

Weyl's own principal contribution to the subject was his 'unified field theory' of gravitation and electricity—the beginning of the quest on which Einstein spent so many fruitless years. The two papers of Weyl on the subject (31, 32, 1918) have been more influential in differential geometry than in relativity theory. Weyl took up Levi-Cività's idea (1917) of the 'parallel displacement' of a vector, but made the decisive innovation of freeing it entirely from dependence on a Riemann metric. An infinitesimal *affine structure* on a differentiable n-manifold is determined, in a given co-ordinate frame, by the choice of n^3 functions Γ^i_{jk} and the parallel vector $(\xi^i + \mathrm{d}\xi^i)$ at $(x^i + \mathrm{d}x^i)$ to (ξ^i) at (x^i) is then defined by

$$\mathrm{d}\xi^i = -\sum \Gamma^i_{rs}\,\xi^r \mathrm{d}x^s.$$

Upon this basis definitions of geodesics and of curvature can be constructed in the usual way. This was the starting point of the rapid development of projective differential geometry ('geometry of paths') which took place, particularly in America under the leadership of O. Veblen, after the first world war. It also greatly clarified the geometrical theory of Lie groups, in the works of E. Cartan and others.

However, Weyl's objective in these two papers was not pure affine geometry, but what he called a 'purely infinitesimal' metric geometry. The Levi-Cività parallel displacement on a Riemannian manifold is obtained by taking Γ^i_{rs} to be $\{{}^{\,i}_{rs}\}$, in the usual notation; but Weyl found it illogical to confine comparisons of *direction* to the infinitesimal, while allowing comparison of *lengths* at a distance. The quadratic form $\Sigma g_{ik}\xi^i\xi^k$, he declared, should be used only to compare vectors at the same point. For other comparisons the space must be *gauged*, i.e., a unit vector must be chosen (arbitrarily at each point, apart from continuity); and only those relations are geometrically significant which are independent of the 'gauging'. This led to the definition of a *gauge-structure* on a manifold as a covariant vector-field ϕ_i. If a Riemann metric, g_{ik} is also given, the *square-length*, l, of a vector ξ^i is still given by $l = \Sigma g_{ik}\xi^i\xi^k$, but *congruent* vectors at (x^i) and $(x^i+\mathrm{d}x^i)$ have lengths l and $l+\mathrm{d}l$, where

$$\mathrm{d}l = -l\sum_i \phi_i \mathrm{d}x^i$$

To make this definition invariant under a change of gauge in which the unit vector at (x) is multiplied by $\lambda(x)$, the 'gauge-vector' must undergo the transformation

$$\phi_i \to \phi_i + \frac{\partial\lambda}{\partial x^i} \tag{5}$$

Gauge invariance accordingly means that only relations invariant under this transformation are geometrically significant. The assumption that parallel vectors are congruent leads to the affine connexion

$$\Gamma^i_{rs} = \{{}^{\,i}_{rs}\} + \tfrac{1}{2}(\delta^i_r\phi_s + \delta^i_s\phi_r - g_{rs}\phi^i)$$

Weyl proposed to identify the ϕ_i with the electromagnetic potentials, which, since they appear in physics only in the combination $\partial\phi_i/\partial x_k - \partial\phi_k/\partial x_i$, do indeed exhibit invariance under (5); and he succeeded in deducing both Einstein's and Maxwell's equations from a single minimum principle. In a note appended to the paper (32) Weyl printed a short criticism by Einstein, who referred to the fact that, just as in a curved space, parallel displacement round a circuit may change direction, so it may also change length in a Weyl space. The size of fundamental particles would accordingly on Weyl's theory depend on their history, and chemically pure elements would not exist. It is doubtful if Weyl's relativistic interpretation of his geometry ever surmounted this objection and it was finally abandoned in favour of a quantum-theoretic one, which is now generally accepted (see 149, 1950). Within the framework of general relativity the constant exponent of the arbitrary phase-factor $e^{-i\lambda}$ of the wave-function ψ must be replaced by a function $\lambda(x)$. To preserve invariance the differential operator $\frac{\partial}{\partial x^k}$ must then be replaced by $\frac{\partial}{\partial x^k}+i\phi_k$, where ϕ_k is to be changed to $\phi_k+\frac{\partial\lambda}{\partial x^k}$ when ψ is multiplied by $e^{-i\lambda}$. From its formal identity with (5) this transformation is still called the 'gauge-change', though the corresponding invariance now expresses the conservation of electric charge. In this form the principle of gauge invariance has been a useful tool in quantum electrodynamics. It requires, for example, that the mass of a photon be rigorously zero even when its interaction with the virtual electron-pair field is taken into account, a condition which helps to fix the meaning of the singular self-energy terms which would otherwise be undefined.

In the papers 83-85, on gravitation and the electron Weyl took up the notion introduced by Einstein in one of his attempts at a 'unified field theory' of attaching a local set of axes (*Vierbein*) to each point of space-time. By using this method to express the relation between a spinor-field and the metric of general relativity, Weyl found a natural interpretation of the Vierbein in terms of the spin, which has proved fruitful in the hands of later workers. He also pointed out in these papers the possibility of representing a fermion field by a pair of 2-component spinors distinguished by definite, and opposite, senses of rotation which on spatial reflexion go over into each other. When the mass is zero the 4-component wave equation degenerates into two uncoupled 2-component equations describing these two oriented spinors. This non-conservation of parity, thought at first to be little more than a mathematical curiosity, has turned out to be an essential property of the so-called weak interactions between fermions of small mass ('leptons'). It is now established that neutrinos are indeed describable by the 2-component formalism introduced by Weyl.

From relativity Weyl turned, by a natural transition, to the problem of finding the 'inner reason' for the structure of general metric space, i.e., deducing the Riemann assumption of a metric based on a quadratic form from axioms about the group of 'movements' in the space. For the classical constant-curvature spaces Helmholtz had characterized the group of movements as the smallest which allows free mobility, i.e., contains just one element which carries a point, a directed line through the point, and so on, into another arbitrary system of the same kind. He sketched a proof, which was made exact by Lie, that such a group coincides with the group of linear transformations leaving a quadratic form invariant. Weyl's problem was to formulate and prove a corresponding theorem for infinitesimal geometry. This he did by analyzing (in 55, 1923) the meaning of the assumption that the metric, i.e., the group of movements, uniquely determines the affine connexion, and he showed that this assumption and the conservation of volume suffice to characterize the group of infinitesimal rotations at a point as the set of linear transformations that leave a non-degenerate quadratic differential form invariant.

From the Lie-Helmholtz space-problem Weyl's attention soon moved to the general problem of the *representation of continuous groups:* the transition is marked by a paper (56, 1923) on the characterization of δ_n, the group of orthogonal transformations of determinant 1 in complex n-space.

The three papers 67 (1925-1926), which Weyl himself considered to be his greatest single contribution to mathematics, are concerned with the representations of a semisimple Lie group $\mathbf{a}$ by linear transformations of a finite dimensional vector space V over the complex field. Such a representation is a function $\rho(u)$ defined for all u in $\mathbf{a}$ and satisfying the law $\rho(uv) = \rho(u)\rho(v)$, the $\rho(u)$ being linear transformations mapping V on to itself, or alternatively, non-singular complex matrices of a fixed size. Two representations ρ, ρ^* in matrix form are called equivalent if there exists a non-singular matrix α independent of u such that $\rho^*(u) = \alpha^{-1}\, \rho(u)\alpha$ for all u in $\mathbf{a}$, and the prime object is to obtain all possible representations of the given group $\mathbf{a}$ to within equivalence. For a finite group, this had been attained in principle by the work of Frobenius, Burnside and Schur in the period 1895-1905. Weyl's results, completed in an essential point by the famous Peter-Weyl Theorem (see below), go a long way towards solving the corresponding problem for semisimple Lie groups, and effectively the whole way for compact Lie groups. He obtained explicit formulae for the characters and degrees of the irreducible representations by a powerful combination of the infinitesimal methods of E. Cartan, the method of invariant integration over the group-manifold of a compact Lie group (first used in a special case by Hurwitz and later by Schur) and topological considerations.

Up to this time Lie groups had been considered almost exclusively from a local, and mainly from an infinitesimal, point of view. Weyl's work contains the first important contributions to the global study of Lie groups and, as such, has been the stimulus to numerous later investigations. For every such group $\mathbf{a}$, the infinitesimal operations form a vector space $\mathbf{a}^\circ$ of the same dimension as $\mathbf{a}$; and the operation of commutation $u^{-1}v^{-1}uv$ in $\mathbf{a}$ gives rise in $\mathbf{a}^\circ$ to a corresponding 'bracket' operation $[x,y]$.

The local structure of $\mathbf{a}$ is reflected in the algebraic structure of $\mathbf{a}^\circ$, the Lie algebra of $\mathbf{a}$. In particular, $\mathbf{a}$ is semisimple if and only if $\mathbf{a}^\circ$ has no soluble ideal $\neq 0$. The trace $\phi_2(x)$ of the transformation $y \rightarrow [[y, x], x]$ of $\mathbf{a}^\circ$ is an invariant quadratic form associated with $\mathbf{a}$, and E. Cartan showed that $\mathbf{a}$ is semisimple if and only if ϕ_2 is non-singular. In his thesis of 1894, Cartan also showed how to transform a semisimple Lie algebra $\mathbf{a}^\circ$ over the complex field into a standard form, with a basis consisting of an Abelian sub-algebra $\mathbf{h}$ together with vectors e_α, where α runs through certain linear forms in the co-ordinates of $\mathbf{h}$. Weyl derived these and many other of the results of Cartan in a particularly elegant way.

For the success of his method it is necessary to consider the real forms of $\mathbf{a}^\circ$ obtained by choosing a basis for $\mathbf{a}^\circ$ (over the complex field) with respect to which all the multiplication constants are real. By some very ingenious calculations, Weyl showed that such a basis can always be found with the additional property that the quadratic form ϕ_2, when restricted to arguments with real co-ordinates, is positive definite; and that a real form with this property is unique to within isomorphism. It is called the compact real form of $\mathbf{a}^\circ$, because the associated adjoint group $\tilde{\mathbf{a}}_u$, since it consists of transformations leaving invariant a positive definite quadratic form, is necessarily compact in the topological sense. But for the purposes of representation theory it is necessary to consider the simply connected covering group $\mathbf{a}_u$ of $\tilde{\mathbf{a}}_u$. By showing that the elements of $\tilde{\mathbf{a}}_u$ which have coincident eigenvalues form a submanifold with dimension three less than that of $\tilde{\mathbf{a}}_u$ itself, Weyl arrived at a fundamental result: the universal covering group $\mathbf{g}$ of a compact semisimple Lie group $\tilde{\mathbf{g}}$ covers $\tilde{\mathbf{g}}$ only a finite number of times and hence is itself compact. This is in striking contrast to the situation which holds for the toral groups, for example the group of rotations of a circle: here, the universal covering group is no longer compact.

The essence of Weyl's method was to pass from the given semisimple group $\mathbf{a}$ to the universal covering group $\mathbf{a}_u$ of the adjoint group $\tilde{\mathbf{a}}_u$ associated with the compact real form of the complex Lie algebra $\mathbf{a}^\circ$ of $\mathbf{a}$, and to infer the properties of representations of $\mathbf{a}$ from those of $\mathbf{a}_u$ by algebraic arguments. The problem was thus reduced to a discussion of the representations of the simply connected and compact Lie group $\mathbf{a}_u$. For a compact group there is available the powerful operation of integrating over the group manifold—the natural analogue of summation over the elements of a finite group. Weyl established very simply the existence of an invariant volume element on any compact Lie group, with respect to which the volume of the whole group is finite. The notion of an invariant integration over a group had first been applied by Hurwitz to the rotation group and by Schur to the representations of the real orthogonal groups. Schur proved the complete reducibility of the representations and the orthogonality relations for the characters of these groups. Weyl extended Schur's results systematically to all the compact Lie groups. More important still, by bringing these global calculations into relation with the fundamental results of E. Cartan on the representations of semisimple Lie algebras, he was able to unify the global and infinitesimal points of view. In particular, he expressed the characteristics and degrees of all possible irreducible representations of a semisimple Lie group $\mathbf{a}$ very elegantly in terms of quantities derived from the geometry of the Lie algebra $\mathbf{a}^\circ$, or more precisely, of the so-called Cartan subalgebra $\mathbf{h}$ referred to above. Weyl's results are in principle subject to assumptions of differentiability since he considered only such representations of $\mathbf{a}$ as may be deduced by integration from a representation $h \to H$, $e_\alpha \to E_\alpha$ of $\mathbf{a}^\circ$ (where $x \to X$, $y \to Y$ implies that $[x,y] \to XY - YX = [X,Y]$). The simplifications and extensions which Weyl thus made to the results of Cartan depended largely on the introduction of a certain finite group S of transformations of $\mathbf{h}$, now known as the Weyl group. This Weyl group has played a fundamental part in subsequent work on the topology of compact Lie groups, by Chevalley and others.

To complete the analogy between finite groups and compact Lie groups it was necessary to prove the completeness of the system of orthogonal functions formed by the coefficients of the inequivalent irreducible unitary representations. This was done in the paper 73 (1927) (with F. Peter), by applying E. Schmidt's methods in the theory of integral equations to a discussion of the eigenfunctions of Hermitian kernels of the form $K(s,t) = \int x(sr^{-1})\bar{x}(tr^{-1})\mathrm{d}r$, where x is a continuous function on this group. The finite dimensional space of eigenfunctions which belong to a given eigenvalue is transformed into itself by the translations of G, and in this way one obtains unitary representations of the group. The Fourier coefficients of x for any given representation can be obtained from a suitable kernel in this way; and the nearer the integral operator represented by the kernel approaches to the identity, the more representations will arise from it. By approximating the identity, one gets all the representations. Thus it is shown that every continuous function $x(s)$ defined on G can be uniformly approximated by a finite linear combination of the coefficients $e_{ik}(s)$ of the unitary irreducible representations of G, only terms being used for which the corresponding Fourier coefficient $(x, e_{ik}) \neq 0$; and a similar result is proved on uniform approximation of class-functions on G by linear combinations of irreducible characters.

Weyl returned to this subject in the paper 99 (1934), and proved an analogous result for the functions $f(P)$, where P varies on a 'homogeneous' manifold π, that is, a compact parametric manifold on which a compact Lie group σ operates as a group of continuous transformations.

The Peter–Weyl paper (73, 1927) preceded by only a few years the construction by Haar in 1933 of a left-invariant measure on any (separable) locally compact topological group. For compact groups, the Haar measure is also right-invariant, and the methods of Peter and Weyl could be carried over with scarcely any change to this more general case. Their work marks a decisive forward step in group-analysis, and points the way to the theory of almost periodic functions on a group, due to von Neumann, and more distantly to the modern theory of representations of locally compact topological groups by unitary transformations of a Hilbert space.

The work of Weyl on continuous groups is especially notable for the explicit algebraic conclusions to which he pushed the general theory in particular cases. The connexion between representations of the general linear groups $\mathbf{a}_n$ and those of the symmetric groups π_f was discovered by Schur. Weyl presented this connexion in a particularly elegant way in the papers 63, 64, 79 and in both his books on group-theory (77 and 115). If T is the space of tensors of rank f on V, then both $\mathbf{a}_n$ and π_f may be regarded as acting on T, the elements of π_f permuting the f tensor suffixes. Let A and Π be the linear envelopes of the transformations induced on T by $\mathbf{a}_n$ and π_f, respectively. Weyl showed very simply that A consists precisely of all bisymmetric transformations of T, that is, all those whose matrix coefficients $a(i_1, \ldots, i_f; j_1, \ldots, j_f)$ are unaffected by an arbitrary permutation of 1, 2, . . ., f. Thus the algebra

A is the commutator algebra of Π. By the standard reciprocity theorem for semisimple algebras of linear transformations, it follows that the irreducible representations of A and Π are in one-to-one correspondence. Since the former correspond to the irreducible components of the representation of $\mathbf{a}_n$ on T, one obtains these components by imposing on T suitable conditions of symmetry, derivable from the well-established representation theory of the finite group π_f. This connexion Weyl made perfectly explicit by the use of A. Young's idempotent symmetry operators. He showed that if $\mathbf{a}_n$ is replaced by its unimodular subgroup, all the irreducible representations may be obtained in this way: each irreducible representation arises from a minimal symmetry class of tensors.

In his book on quantum mechanics, Weyl applied this theory to give a profound analysis of the theory of non-combining term-systems in an atom with several valency electrons, and the splitting of terms under weak interactions. This theory had been originated by Wigner and Hund. Owing to the spin of the electrons and the Pauli exclusion principle, the tensor space which has to be split up into irreducible components in this problem has a more elaborate structure than the space T referred to above. Weyl returned later to the same topic to give a particularly simple exposition in the paper 143 (1949).

The initial development of quantum mechanics had indeed been simultaneous with Weyl's work on continuous groups; and he was quick to seize the opportunity presented by the new theory for the application of group-theoretical methods. Not only the symmetric groups, but also the rotation group and the Lorentz group occupy an important place in his exposition. For example, angular momentum, the selection and intensity rules, multiplet structure and the anomalous Zeeman effect appear as aspects of the study of the group of rotations. Entirely original is his interpretation, on the basis of Heisenberg's commutation relations for canonical variables p_a, q_a, of quantum kinematics in terms of an Abelian group of unitary ray-rotations in system space.

Among Weyl's contributions to the study of the orthogonal groups, paper 107 (1935) on spinors in n dimensions, written jointly with R. Brauer, remains classical. The orthogonal group δ_n of degree n is not simply connected and possesses two-valued representations. The most essential of these, from which the others may be derived, is a representation of degree 2^ν, where $\nu = [\frac{1}{2}n]$. This had been discovered by E. Cartan using infinitesimal methods. The case $n = 4$ underlies Dirac's theory of the spinning electron. Brauer and Weyl constructed these spinor representations by purely algebraic methods, starting from the Clifford algebra associated with the quadratic form defining δ_n. This has great advantages of simplicity and directness and also makes it easy to deal with the case of quadratic forms of arbitrary inertial index.

In his book *The classical groups*, Weyl gave a connected account of the representations and invariants of these groups. His concept of an invariant is much wider than that which had dominated the theory of algebraic invariants in its heyday in the second half of the nineteenth century. If $x, y, \ldots$ are variable vectors chosen from representation spaces of a linear group $\mathbf{g}$, then an invariant of $\mathbf{g}$ is a polynomial $f(x,y, \ldots)$ in the co-ordinates of $x,y, \ldots$ such that $f(x,y \ldots) = f(\sigma x, \sigma y, \ldots)$ for all σ in $\mathbf{g}$. Relative invariants are defined in a similar way. In the classical period of invariant theory, one normally considered only the representation spaces provided by algebraic forms of a given degree in the co-ordinates of the space V on which $\mathbf{g}$ acts. Weyl's point of view is not only more general, but is clearly the natural one to take today when, largely through his own efforts, the representation theory of the classical groups has been so adequately developed. The result is that Weyl has succeeded in bringing this attractive theory once more into the main stream of algebraic thought.

Mathematical logic. In Hilbert's development of axiomatic mathematics two stages can be seen. The first is the setting up of branches of mathematics as theories of pure structure, that is to say, as theories about sets in which some subsets are distinguished, in accordance with certain conditions. The distinguished subsets may be given such picturesque names as 'open set', 'straight line', or 'pair of elements and their product', but it is the conditions they satisfy, not their names, that make one subject differ from another. The second stage is the formalization of set theory itself, which in the first stage was treated 'naively'.

Axiomatic theories in the first sense were quite congenial to Weyl, who, as we have seen, was himself the first to give recognizable axiomatic characterizations of two central concepts of modern mathematics, topological neighbourhood-spaces and cell-complexes, and in the *Mathematical analysis of the space-problem* found axioms for Riemannian geometry. No logical problems need be posed by such theories, provided that illustrative examples or 'models' can be made out of the real numbers, and provided, of course, that the real numbers are accepted as secure. Hilbert's treatment of the second stage, which amounted to reducing mathematics to the properties of the grammar of the sentences expressing its theorems, was highly repugnant to Weyl. He perhaps took too seriously the comparison sometimes made between making a proof according to Hilbert's 'rules of procedure', and playing a game of chess. The object of the comparison is to illustrate the surprising and important fact that formal logical systems can be described objectively without reference to their intended meaning, but Weyl saw in this a degradation of mathematics. 'Hilbert's mathematics may be a pretty game with formulae, more amusing even than chess; but what bearing does it have on cognition, since its formulae admittedly have no material meaning by virtue of which they could express intuitive truths?' (141, 1949, p. 61). He quotes with approval Brouwer's aphorism 'To the question, where shall mathematical rigour be found, the two parties give different answers. The intuitionist says: in the human intellect, the formalist: on paper.'

His own first incursion into the field was in the monograph *Das Kontinuum*, and he never departed greatly from the position he there took up (though he published a more extended exposition in 41, 1921). A characteristic opening paragraph declared his purpose. 'In this little book I am not concerned to disguise the "solid rock" on which the house of analysis is built with a wooden platform of formalism, in order to talk the reader into believing at the end that this platform is the true foundation. What will be propounded is rather the view that the house is largely built on sand. I believe I can replace this shaky part of the foundation by strong and reliable supports, but they will not carry everything that is nowadays generally believed to be secure. The rest I abandon: I can see no other possibility.'

The 'sandy part' was the part of mathematics which (he said) involves a 'vicious circle', namely the kind of definition called 'impredicative' by Russell, who also saw here a danger to the stability of mathematics. As a measure of protection against the appearance of 'semantic' paradoxes Russell had enunciated the principle: 'No totality can contain members defined in terms of itself.' This principle appears to be violated by the usual definition of the *least upper bound*, sup E, of a set, E of real numbers, when these are defined as Dedekind 'segments' (lower classes of Dedekind cuts); namely, sup $E = \bigcup E$, the union of all the members of E. It is easily shown that $\bigcup E$, a set of rational numbers, is a segment. But its definition involves a reference to the 'totality' of all real numbers, of which it is itself a member, since E is in general defined as the set of all real numbers having a certain property.

It was to give formal expression to his principle that Russell introduced the 'ramified' theory of types, of which Weyl's *Kategorien* and *Stufen*, in *Das Kontinuum*, are a version. The almost complete avoidance of formulae by Weyl in describing what is really a formal device, depending heavily on the precise rules of substitution for variables, makes it difficult to discern the details of his scheme, but as in other type-theories, one can distinguish two stages. To avoid the cruder 'Russell' paradox it is supposed that objects and classes themselves have a numerical type, and that the members of a class are of lower type (in some versions, of type exactly one lower) than the class itself. The 'ramified' theories go behind the class to the formula (with a free variable x) that defines it. The type of a class-with-definition is to be higher, in some ordering, not necessarily linear, than that of any variable that occurs, *free or bound*, in the definition. There will accordingly be (for example) real numbers of various types, according to the complexity of their definitions. The definition of sup E is no longer open to the objection of being impredicative since it defines sup E as a real number of type $\alpha+1$ in terms of the totality of real numbers of type α: but the proliferation of real numbers of different types makes analysis quite intractable and led Russell to the desperate expedient of the Axiom of Reducibility, which simply postulates that for every sentence with a single free variable, x, of level α, there exists a 'first-order' sentence defining the same class, that is a sentence of the lowest type possible for sentences with the free variable x. Weyl rejected this way out of the difficulty. 'Russell, in order to extricate himself from the affair, causes reason to commit hara-kiri, by postulating [the existence of an equivalent first order sentence] in spite of its lack of support by any evidence' (141, 1949, p. 50).

His own cure was the drastic one of allowing only 'first order' definitions, and throwing away the parts of mathematics that failed to survive the purge. This means that bounded sets of real numbers need not have least upper bounds, and that we may not, in general, form the set, $\mathbf{P}(E)$, of all subsets of a given set. He carried out in detail, in the book, the development of analysis as far as it would go on this basis. His set theory was of a genetic kind, only such sets being admitted as could be built up from 'ground categories' by the use of allowed principles of construction. It thus resembled the Zermelo system (to which he refers) later developed by von Neumann; but in place of the more powerful Zermelo operations, such as formation of $\mathbf{P}(E)$, he uses only the combination of Boolean operations and quantification of type-0 variables, by means of a recursive iteration scheme. The theory of real numbers to which this leads is of the sort that has been made familiar by the Intuitionists. The extent of the sacrifice involved is much more accurately known at the present day. The movement of logic is now towards a re-interpretation of classical proofs in a constructive sense, rather than a policy of voluntarily jettisoning certain of the most powerful instruments of proof, which is unlikely to recommend itself to mathematicians in general. Nevertheless a return to the 'naive' acceptance of the axioms of classical set-theory as self-evident truths, on which we can confidently build up mathematics, is now out of the question; and in this change of opinion Weyl's writings certainly played an important part. Brouwer's analysis goes deeper, but his theories are to many impenetrable. It was Weyl's advocacy of the intuitionist views and his clear and attractive expositions of them, in his papers and in the book (71), later translated and revised as (141), that first made them accessible to many mathematicians, and turned the revolutionary doctrine of his time into the orthodoxy of today.

I wish to acknowledge my indebtedness to H. Davenport, F.R.S., P. Hall, F.R.S., G. E. H. Reuter and L. Rosenfeld, who have contributed a substantial part of the material used in the scientific part of the memoir; to Professor König and Professor Plancherel for their personal recollections of Weyl in Göttingen; to Professor Eckmann, Weyl's colleague at Zurich who allowed me the use of the 'Selecta' bibliography; and to Frau Ellen Weyl for her help in many ways.

M. H. A. Newman

BIBLIOGRAPHY

(1) 1908. Singuläre Integralgleichungen mit besonderer Berücksichtigung des Fourierschen Integraltheorems. *Dissertation Göttingen.*

(2) 1908. (With F. Jerosch.) Über die Konvergenz von Reihen, die nach periodischen Funktionen fortschreiten. *Math. Ann.* **66**, 67-80.

(3) 1908. Singuläre Integralgleichungen. *Math. Ann.* **66**, 273-324.

(4) 1909. Über die Konvergenz von Reihen, die nach Orthogonalfunktionen fortschreiten *Math. Ann.* **67**, 225-245.

(5) 1909. Über beschränkte quadratische Formen, deren Differenz vollstetig ist. *Rendiconti Mat. Palermo.* **27**, 373-392.

(6) 1909. Über gewöhnliche lineare Differentialgleichungen mit singulären Stellen und ihre Eigenfunktionen. *Nachr. Ges. Wiss. Göttingen.* pp. 37-63.

(7) 1910. Über gewöhnliche lineare Differentialgleichungen mit singulären Stellen und ihre Eigenfunktionen (2. Note). *Nachr. Ges. Wiss. Göttingen.* pp. 442-467.

(8) 1910. Über gewöhnliche Differentialgleichungen mit Singularitäten und die zugehörigen Entwicklungen willkürlicher Funktionen. *Math. Ann.* **68**, 220-269.

(9) 1910. Über die Definitionen der mathematischen Grundbegriffe. *Math. naturwis. Blätter.* **7**, 93-95 and 109-113.

(10) 1910. Die Gibbssche Erscheinung in der Theorie der Kugelfunktionen. *Rendiconti Mat. Palermo.* **29**, 308-323 and 377-407.

(11) 1911. Zwei Bemerkungen über das Fouriersche Integraltheorem. *Jber. dtsch. MatVer.* **20**, 129-141 and 339.

(12) 1911. Über die asymptotische Verteilung der Eigenwerte. *Nachr. Ges. Wiss. Göttingen.* pp. 110-117.

(13) 1911. Konvergenzcharakter der Laplaceschen Reihe in der Umgebung eines Windungspunktes. *Rendiconti Mat. Palermo.* **32**, 118-131.

(14) 1912. Henri Poincaré. *Math. naturwis. Blätter.* **9**, 161-163.

(15) 1912. Das asymptotische Verteilungsgesetz der Eigenwerte linearer partieller Differentialgleichungen. *Math. Ann.* **71**, 441-479.

(16) 1912. Über die Abhängigkeit der Eigenschwingungen einer Membran von deren Begrenzung. *Crelle's J.* **141**, 1-11.

(17) 1912. Über das Spektrum der Hohlraumstrahlung. *Crelle's J.* **141**, 103-181.

(18) 1913. *Die Idee der Riemannschen Fläche*, Leipzig.

(19) 1913. Über die Randwertaufgabe der Strahlungstheorie. *Crelle's J.* **143**, 177-202.

(20) 1914. Über ein Problem aus dem Gebiete der diophantischen Approximationen. *Nachr. Ges. Wiss. Göttingen.* pp. 234-244.

(21) 1914. Sur une application de la théorie des nombres à la mécanique statistique. *Enseign. math.* **16**, 455-467.

(22) 1915. Das asymptotische Verteilungsgesetz der Eigenschwingungen eines beliebig gestalteten elastischen Körpers. *Rendiconti Mat. Palermo.* **39**, 1-50.

(23) 1916. Über die Gleichverteilung von Zahlen mod. Eins. *Math. Ann.* **77**, 313-352.

(24) 1916. Strenge Begründung der Charakteristikentheorie auf zweiseitigen Flächen. *Jber. dtsch. MatVer.* **25**, 265-278.

(25) 1916. Über die Bestimmung einer geschlossenen konvexen Fläche durch ihr Linienelement. *Vierteljahrsschrift Zürich.* **61**, 40-72.

(26) 1917. Le problème de l'analysis situs. *Enseign. math.* **19**, 95-96.

(27) 1917. Über die Starrheit der Eiflächen und konvexer Polyeder. *S.B. preuss. Akad. Wiss.* pp. 250-266.

(28) 1917. Bemerkungen zum Begriff des Differentialquotienten gebrochener Ordnung. *Vierteljahrsschrift Zürich.* **62**, 296-302.

(29) 1917. Zur Gravitationstheorie. *Ann. Phys. Lpz.* **54**, 117-145.
(30) 1918. *Raum-Zeit-Materie.* Berlin.
(31) 1918. Reine Infinitesimalgeometrie. *Math. Z.* **2**, 384-411.
(32) 1918. Gravitation und Elektrizität. *S.B. preuss. Akad. Wiss.* pp. 465-480.
(33) 1919. *Kommentar zu Riemann's 'Über die Hypothesen. . .'* Berlin.
(34) 1919. Der circulus vitiosus in der heutigen Begründung der Analysis. *Jber. dtsch MatVer.* **28**, 85-92.
(35) 1919. Über die statischen kugelsymmetrischen Lösungen von Einsteins 'kosmologischen' Gravitationsgleichungen. *Phys. Z.* **20**, 31-34.
(36) 1919. Eine neue Erweiterung der Relativitätstheorie. *Ann. Phys. Lpz.* **59**, 101-133.
(37) 1919. Bemerkung über die axialsymmetrischen Lösungen der Einsteinschen Gravitationsgleichungen. *Ann. Phys. Lpz.* **59**, 185-188.
(38) 1919. Ausbreitung elektromagnetischer Wellen über einem ebenen Leiter. *Ann. Phys. Lpz.* **60**, 481-500.
(39) 1920. Das Verhältnis der kausalen zur statistischen Betrachtungsweise in der Physik. *Schweiz. Med. Wschr.* **50**, 737-741.
(40) 1920. Elektrizität und Gravitation. *Phys. Z.* **21**, 649-650.
(41) 1921. Über die neue Grundlagenkrise der Mathematik. *Math. Z.* **10**, 39-79.
(42) 1921. Zur Abschätzung von $\zeta(1 + ti)$. *Math. Z.* **10**, 88-101.
(43) 1921. Zur Infinitesimalgeometrie: Einordnung der projektiven und konformen Auffassung. *Nachr. Ges. Wiss. Göttingen.* pp. 99-112.
(44) 1921. Bemerking über die Hardy-Littlewoodschen Untersuchungen zum Waringschen Problem. *Nachr. Ges. Wiss. Göttingen.* pp. 189-192.
(45) 1921. Das Raumproblem. *Jber. dtsch. MatVer.* **30**, 92-93.
(46) 1921. Über die physikalischen Grundlagen der erweiterten Relativitätstheorie. *Phys. Z.* **22**, 473-480.
(47) 1921. Feld und Materie. *Ann. Phys. Lpz.* **65**, 541-563.
(48) 1921. Electricity and gravitation. *Nature, Lond.* **106**, 800-802.
(49) 1922. Die Einzigartigkeit der Pythagoreischen Massbestimmung. *Math. Z.* **12**, 114-146.
(50) 1922. Zur Infinitesimalgeometrie: p-dimensionale Fläche im n-dimensionalen Raum. *Math. Z.* **12**, 154-160.
(51) 1922. Neue Lösungen der Einsteinschen Gravitationsgleichungen. *Math. Z.* **13**, 142-145.
(52) 1922. Die Relativitätstheorie auf der Naturforscherversammlung. *Jber. dtsch. MatVer.* **31**, 51-63.
(53) 1922. Das Raumproblem. *Jber. dtsch. MatVer.* **31**, 205-221.
(54) 1923. *Raum-Zeit-Materie.* (5th and last ed.) Berlin.
(55) 1923. *Mathematische Analyse des Raumproblems.* Berlin.
(56) 1923. Zur Charakterisierung der Drehungsgruppe. *Math. Z.* **17**, 293-320.
(57) 1923. Zur allgemeinen Relativitätstheorie. *Phys. Z.* **24**, 230-232.
(58) 1923. Repartición de corriente en una red conductora. (Introducción al análisis situs combinatorio). *Revista Mat. Hispano-Americana.* **5**, 153-164.
(59) 1923. Análisis situs combinatorio. *Revista. Mat. Hispano-Americana.* **5**, 43p, **6**, 1-9, and 33-42.
(60) 1924. *Was ist Materie?* Berlin.
(61) 1924. Randbemerkungen zu Hauptproblemen der Mathematik. *Math. Z.* **20**, 131-150.
(62) 1924. Zur Theorie der Darstellung der einfachen kontinuierlichen Gruppen. (Aus einem Schreiben an Herrn. I. Schur). *S.B. preuss. Akad. Wiss.* pp. 338-345.
(63) 1924. Das gruppentheoretische Fundament der Tensorrechnung. *Nachr. Ges. Wiss Göttingen.* pp. 218-224.
(64) 1924. Über die Symmetrie der Tensoren. *Rendiconti Mat. Palermo.* **48**, 29-36.
(65) 1924. Massenträgheit und Kosmos. Ein Dialog. *Naturwissenschaften,* **12**, 197-204.

(66) 1925. Die heutige Erkenntnislage in der Mathematik. *Symposion.* **1**, 1-32.

(67) 1925-26. Theorie der Darstellung kontinuierlicher halbeinfacher Gruppen durch lineare Transformationen. I, II, III und Nachtrag. *Math. Z.* **23**, 271-309; **24**, 328-376; **24**, 377-395; **24**, 789-791.

(68) 1926. Zur Darstellungstheorie und Invariantenabzählung der projektiven, der Komplex-und der Drehungsgruppe. *Acta Math.* **48**, 255-278.

(69) 1926. Elementare Sätze über die Komplex-und die Drehungsgruppe. *Nachr. Ges. Wiss. Göttingen.* pp. 235-243.

(70) 1926. Beweis des Fundamentalsatzes in der Theorie der fastperiodischen Funktionen. *S.B. preuss. Akad. Wiss.* pp. 211-214.

(71) 1927. *Philosophie der Mathematik und Naturwissenschaften.* Munich.

(72) 1927. Integralgleichungen und fastperiodische Funktionen. *Math. Ann.* **97**, 338-356.

(73) 1927. (With F. Peter.) Die Vollständigkeit der primitiven Darstellungen einer geschlossenen kontinuierlichen Gruppe. *Math. Ann.* **97**, 737-755.

(74) 1927. Sur la représentation des groupes continus. *Enseign. math.* **26**, 226-239.

(75) 1927. Quantenmechanik und Gruppentheorie. *Z. Phys.* **46**, 1-46.

(76) 1927. Strahlbildung nach der Kontinuitätsmethode behandelt. *Nachr. Ges. Wiss. Göttingen.* pp. 227-237.

(77) 1928. *Gruppentheorie und Quantenmechanik.* Leipzig.

(78) 1929. Consistency in mathematics. *Rice Inst. Pamphl.* **16**, 245-265.

(79) 1929. Der Zusammenhang zwischen der symmetrischen und der linearen Gruppe. *Ann. Math. Princeton.* **30**, 499-516.

(80) 1929. Kontinuierliche Gruppen und ihre Darstellungen. *Atti del Congresso Mat. Bologna.* **1**, 233-246.

(81) 1929. (With H. P. Robertson.) On a problem in the theory of groups. *Bull. Amer. Math. Soc.* **35**, 686-690.

(82) 1929. On the foundations of infinitesimal geometry. *Bull. Amer. Math. Soc.* **35**, 716-725.

(83) 1929. Gravitation and the electron. *Proc. Nat. Acad. Sci. Wash.* **15**, 323-334.

(84) 1929. Gravitation and the electron. *Rice Inst. Pamphl.* **16**, 280-295.

(85) 1929. Elektron und Gravitation. *Z. Phys.* **56**, 330-352.

(86) 1929. The spherical symmetry of atoms. *Rice Inst. Pamphl.* **16**, 266-279.

(87) 1929. The problem of symmetry in quantum mechanics. *J. Franklin Inst.* **207**, 509-518.

(88) 1930. Felix Kleins Stellung in der mathematischen Gegenwart. *Naturwissenschaften.* **18**, 4-11.

(89) 1930. Redshift and relativistic cosmology. *Phil. Mag.* **9**, 936-943.

(90) 1930-31. Zur quantentheoretischen Berechnung molekularer Bindungsenergien. *Nachr. Ges. Wiss. Göttingen.* pp. 285-294 (1930), pp. 33-39 (1931).

(91) 1931. *Die Stufen des Unendlichen.* Jena.

(92) 1931. Über das Hurwitzsche Problem der Bestimmung der Anzahl Riemannscher Flächen von gegebener Verzweigungsart. *Commentarii math.* **3**, 103-113.

(93) 1931. Geometrie und Physik. *Naturwissenschaften.* **19**, 49-58.

(94) 1932. Zu David Hilberts siebzigsten Geburtstag. *Naturwissenschaften.* **20**, 57-58.

(95) 1932. *The open world.* London.

(96) 1932. Topologie und abstrakte Algebra als zwei Wege mathematischen Verständnisses. *UnterrBl. Math. Naturw.* **38**, 177-188.

(97) 1932. Über Algebren, die mit der Komplexgruppe in Zusammenhang stehen, und ihre Darstellungen. *Math. Z.* **35**, 300-320.

(98) 1932. (With G. Rumer and E. Teller.) Eine für die Valenztheorie geeignete Basis der binären Vektorinvarianten. *Nachr. Ges. Wiss. Göttingen.* pp. 499-504.

(99) 1934. Harmonics on homogenous manifolds. *Ann. Math. Princeton.* **35**, 486-499.

(100) 1934. On generalized Riemann matrices. *Ann. Math. Princeton.* **35**, 714-729.

(101) 1934. Observations on Hilbert's independence theorem and Born's quantization of field equations. *Phys. Rev.* **46**, 505-508.

(102) 1934. Universum und Atom. *Naturwissenschaften.* **22**, 145-149.

(103) 1934. *Mind and nature*. London.
(104) 1935. Emmy Noether. *Scr. math. N.Y.* **3**, 201-220.
(105) 1935. Über das Pick-Nevanlinnasche Interpolationsproblem und sein infinitesimales Analogon. *Ann. Math. Princeton.* **36**, 230-254.
(106) 1935. Geodesic fields in the calculus of variations for multiple integrals. *Ann. Math. Princeton.* **36**, 607-629.
(107) 1935. (With R. Brauer.) Spinors in *n* dimensions. *Amer. J. Math.* **57**, 425-449.
(108) 1935. Elementare Theorie der konvexen Polyder. *Commentarii math.* **7**, 290-306.
(109) 1935. Generalized Riemann matrices and factor sets. *Ann. Math. Princeton.* **37**, 709-745.
(110) 1937. Note on matric algebras. *Ann. Math. Princeton.* **38**, 477-483.
(111) 1937. Commutator algebra of a finite group of collineations. *Duke Math. J.* **3**, 200-212.
(112) 1938. Symmetry. *J. Wash. Acad. Sci.* **28**, 253-271.
(113) 1938. (With J. Weyl.) Meromorphic curves. *Ann. Math. Princeton.* **39**, 516-538.
(114) 1938-39. Mean motion. *Amer. J. Math.* **60**, 889-896, and **61**, 143-148.
(115) 1939. *The classical groups*. Princeton.
(116) 1939. On unitary metrics in projective space. *Ann. Math. Princeton.* **40**, 141-148 and 634-635.
(117) 1939. On the volume of tubes. *Amer. J. Math.* **61**, 461-472.
(118) 1939. Invariants. *Duke Math. J.* **5**, 489-502.
(119) 1940. *Algebraic theory of numbers*. Princeton.
(120) 1940. The ghost of modality. In *Philosophical essays in memory of Edmund Husserl*. Cambridge (Mass.) pp. 278-303.
(121) 1940. The mathematical way of thinking. *Science*. **92**, 437-446.
(122) 1940-42. Theory of reduction for arithmetical equivalence. *Trans. Amer. Math. Soc.* **48**, 126-164 and **51**, 203-231.
(123) 1940. The method of orthogonal projection in potential theory. *Duke Math. J.* **7**, 411-444.
(124) 1941. On the use of indeterminates in the theory of the orthogonal and symplectic groups. *Amer. J. Math.* **63**, 777-784.
(125) 1941-42. Differential equations of some boundary layer problems. *Proc. Nat. Acad. Sci. Wash.* **27**, 578-583; and **28**, 100-102.
(126) 1942. On the differential equations of the simplest boundary-layer problems. *Ann. Math. Princeton.* **43**, 381-407.
(127) 1942.*On the geometry of numbers. *Proc. Lond. Math. Soc.* **47**, 268-289.
(128) 1942. (With R. D. James.) Elementary note on prime number problems of Vinogradoff's type. *Amer. J. Math.* **64**, 539-552.
(129) 1942. (With J. Weyl.) On the theory of analytic curves. *Proc. Nat. Acad. Sci. Wash.* **28**, 417-421.
(130) 1943. *Meromorphic functions and analytic curves*. Princeton.
(131) 1943. On Hodge's theory of harmonic integrals. *Ann. Math. Princeton.* **44**, 1-6.
(132) 1944. Obituary: David Hilbert 1862-1943. *Obit. Not. Roy. Soc.* **4**, 547-553.
(133) 1944. David Hilbert and his mathematical work. *Bull. Amer. Math. Soc.* **50**, 612-654.
(134) 1944. Concerning a classical problem in the theory of singular points of ordinary differential equations. *Act. Acad. Cienc. Lima.* **7**, 21-60.
(135) 1944. Comparison of a degenerate form of Einstein's with Birkhoff's theory of gravitation. *Proc. Nat. Acad. Sci. Wash.* **30**, 205-210.
(136) 1944. How far can one get with a linear field theory of gravitation in flat spacetime? *Amer. J. Math.* **66**, 591-604.
(137) 1945. Fundamental domains for lattice groups in division algebras. *Commentarii math.* **17**, 283-306.
(138) 1946. Encomium (Wolfgang Pauli). *Science*, **103**, 216-218.

---

* Received 1939.

(139) 1946. Mathematics and logic. *Amer. Math. Monthly*, **53**, 2-13.

(140) 1946. Comment on a paper by Levinson. *Amer. J. Math.* **68**, 7-12.

(141) 1949. *Philosophy of mathematics and natural science* (revision of 71). Princeton.

(142) 1949. Wissenschaft als symbolische Konstruktion des Menschen. *Eranos-Jahrbuch* 1948, 375-431.

(143) 1949. Elementary algebraic treatment of the quantum mechanical symmetry problem. *Canad. J. Math.* **1**, 57-68.

(144) 1949. Almost periodic invariant vector sets in a metric vector space. *Amer. J. Math.* **71**, 178-205.

(145) 1949. Inequalities between the two kinds of eigenvalues of a linear transformation. *Proc. Nat. Acad. Sci. Wash.* **35**, 408-411.

(146) 1949. Relativity theory as a stimulus in mathematical research. *Proc. Amer. Phil. Soc.* **93**, 535-541.

(147) 1949. Shock waves in arbitrary fluids. *Commun. Pure Appl. Math.* **2**, 103-122.

(148) 1950. A remark on the coupling of gravitation and electron. *Phys. Rev.* **77**, 699-701.

(149) 1950. 50 Jahre Relativitätstheorie. *Naturwissenschaften.* **38**, 73-83.

(150) 1950. Ramifications, old and new, of the eigenvalue problem. *Bull. Amer. Math. Soc.* **56**, 115-139.

(151) 1950. Elementary proof of a minimax theorem due to von Neumann. (Contributions to the theory of games I.) *Ann. Math. Stud.* **24**, 19-25.

(152) 1950. A half-century of mathematics. *Amer. Math. Mon.* **58**, 523-553.

(153) 1950. Radiation capacity. *Proc. Nat. Acad. Sci. Wash.* **37**, 832-836.

(154) 1952. *Symmetry*. Princeton.

(155) 1952. Kapazität von Strahlungsfeldern. *Math. Z.* **55**, 187-198.

(156) 1952. Die natürlichen Randwertaufgaben im Aussenraum für Strahlungsfelder beliebiger Dimension und beliebigen Ranges. *Math. Z.* **56**, 105-119.

(157) 1953. Über den Symbolismus der Mathematik und mathematischen Physik. *Studium generale*, **6**, 219-228.

(158) 1953. Über die kombinatorische und kontinuumsmässige Definition der Überschneidungszahl zweier geschlossener Kurven auf einer Fläche. *Z. Angewandte Math. Phys.* **4**, 471-492.

(159) 1955. *Die Idee der Riemannschen Fläche.* (Revised edition.) Leipzig.

(160) 1955. Rückblick auf Zürich aus dem Jahre 1930. *Schweizerische Hochschulzeitung.* **28**, 1-8.

(161) 1955. Erkenntnis und Besinnung. (Ein Lebensrückblick). *Studia Philosophica.* **15**, 153-171.

(162) 1956. *Selecta Hermann Weyl.* Basel.

(163) 1957. Address on the Fields Medal Awards. *Proc. Internat. Congress Math. Amsterdam* (1954), **1**, 161-174.

GRAHAM LIBRARY